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Editorial Notes



READERS of ANTIQUITY sometimes complain that we do not publish more articles about British prehistory which provided the material for some outstanding contributions during our first decade. The explanation will emerge, we think, from the answer to a question: What is it that, in the last resort, determines the contents of a number of ANTIQUITY? It is not merely the Editor's personal interests, though naturally these must influence his decisions. The ultimate determining factor is rather the state of archaeology in any particular region. Archaeology is an art which employs a scientific technique, and like all arts it has periods of ebullience when (to quote the dictionary) new discoveries 'issue forth in agitation, like boiling water', and others when hardly a ripple disturbs the surface. We are certainly not in the latter stage, but has the water gone off the boil? Most certainly it has not. New discoveries of first-rate importance, such as the mesolithic habitation-site at Star Carr, continue to be made and are exploited by the most advanced techniques. The essential difference between now and two or three decades ago is surely that the blank periods in British prehistory have largely been filled in. The work of three or four prehistorians has enabled us to see in outline the changing scenes of life for some six or seven thousand years. There are still a few blank periods such as the later mesolithic and the Middle Bronze Age, but the others are now well established.





What remains therefore is the hardly less exciting task of employing new techniques, such as air-photography and radio-carbon dating, to amplify the picture and correct its outlines. The former, now 30 years old, is perhaps hardly quite new, but the conditions of its employment have delayed rapid progress. Thanks to an excellent cooperation between Cambridge University and the Royal Air Force a big blank on the map of Roman Britain is now being filled in. Nine years ago I forecast 'startling discoveries' in sw Scotland, quoting Tacitus' remark that in A.D. 82 Agricola 'manned with troops that part of Britain which faces Ireland' (*Topography of Roman Scotland*, 130). On the Ordnance Survey Map of Roman Britain that region has no Roman sites at all. Now Dr St. Joseph has begun to fill it in, and has published an excellent general résumé of his discoveries. Taking full advantage of the drought of 1949 he has

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found and photographed from the air a number of Roman forts and marching-camps in the valleys of the Nith and the Dee and carried the evidence of Roman military penetration as far west as Gatehouse of Fleet. It is now certain that the counties of Ayr and Wigtown must also contain Roman forts, for the advance would not have stopped short of the sea coast. By kind permission of the Editor and himself we reproduce here (*facing* p. 57) two of the air-photographs illustrating his article (*Journal of Roman Studies*, Vol. XLI, 1951). One shows the plan of an entirely new fort at Glenlochar, Kirkcudbrightshire, with its triple ditches and streets; the other reveals in startling detail the interior buildings of an already known Roman fort at Beckfoot in Cumberland. Both are revealed by crop-marks, in the latter oats. In the middle of the latter photograph, beside the road, can be seen the stone barn with its buttresses. In all Dr St. Joseph has discovered 'six new large forts, nineteen small forts, eleven signal stations or turrets, about sixty temporary camps, detailed plans of several extramural settlements, as well as of five forts, while at almost every major site . . . air photography has contributed fresh information'. This is a record of which British archaeology may well be proud; here certainly is no lack of ebullience.



In excavation, too, Scotland has been very active, as the 6th Report of the Scottish Regional Group shows (copies of this and the next Report to be obtained from Mr R. W. Feachem, F.S.A., 3 South Bridge, Edinburgh 1, for 6d.). Much of it is done with student labour and served for training purposes. We have indeed travelled far from the times, nearly half a century ago, when Hadrian Allcroft lamented the neglect of our native sites. We have not come near to exhausting them, however, for as technique advances we are able to extract more and more information from each of them; indeed the time is ripe for the re-excitation of some of the more important ones, employing our newly acquired skill to amplify the older results. It would be well worth while thus to re-excavate some of Colt Hoare's barrows on Salisbury Plain. But as our knowledge of prehistoric and Romano-British archaeology increases and the blanks are filled, it is likely that excavations in Britain will tend more and more to become training-grounds for archaeologists whose major activities are destined for work elsewhere. The 'painstaking analysis of our ultimate prehistoric slums' is a good preparation for those who will proceed to uncover 'major civilizations and cultures of world-wide significance'. Egypt and India have already profited by such a training; the principles of excavation and field archaeology are the same in all continents.



But ANTIQUITY has also to consider the interests of a large circle of readers, some of whom are not particularly interested in British archaeology. Enthusiasm is infectious, and it may well be that the current enthusiasms of whole-time archaeologists make the most interesting reading. What is it that arouses such enthusiasm? Chiefly it is the opportunity of using the skill they have acquired by training in a region where it has not previously been employed. That always results in new discoveries, and it is the making of discoveries that is the salt of archaeology. To judge from the questions one is asked most people seem to imagine that the archaeologist spends his time going round looking at well-known sites! Actually it is the ones which are not known that attract him. Nothing is so boring as rubbernecking round ruins; nothing so exciting as to set out

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on a voyage of discovery in a region that has not previously been explored, knowing that all ahead is new and unknown.



These things being so, it is inevitable that some of the best copy should come from marginal areas in Africa and Asia. Africa is particularly ebullient at the moment from Khartoum to Cape Town, via Kenya and Angola. With a clear field and exciting new problems to solve some dozen or so enthusiasts are reconstructing the history of the human race in the continent where it may have originated. Others (including the writer) are concerned with fascinating by-ways such as the Southern Christian Kingdom on the Middle Nile, whose history is almost a blank. It is good news that the Commissioner for Archaeology in the Sudan, Mr Peter Shinnie, F.S.A., is starting a new periodical publication (to be called KUSH we believe) in which the many new discoveries made recently will be recorded. He himself has just completed the second and last season's work at Soba, the capital of the Southern Christian Kingdom of Alwa which came to an end (perhaps by gradual decay and abandonment) somewhere between 1317 and 1522.



In the older countries of Europe—older, that is, in archaeological experience—it is an age of maturity and achievement rather than of pioneering. Enough has now been done for big books to be written, summing up the evidence. Such is the fine work of Dr J. G. D. Clark which has just appeared (see p. 86 for full title). In keeping with the times it is concerned with the basic facts of prehistoric life. That in itself is evidence of the maturity of the discipline; the scaffolding is now discarded and we can see the building.



The Scottish Regional Group of the Council for British Archaeology is planning to bring into being an annual Summer School in Archaeology. The first session will be held in Dundee this year, from 8th to 12th August, and the subject chosen as a theme for the Session is 'The Problem of the Picts'. It is intended to hold the School in a different part of Scotland each year, the subject in each case being appropriate to the area selected. For example, the 1953 Session is planned for Dumfries, the theme being 'Roman and Native in Southwest Scotland', while in succeeding years other centres, such as Inverness, Perth or Glasgow will be chosen.

Particulars of the 1952 School may be had from the Hon. Secretary, Scottish Summer School in Archaeology, 3 South Bridge, Edinburgh, 1.

Stratigraphic Excavations in the Forum Romanum

by EINAR GJERSTAD

(Lund University)

IN 1903 and 1904 Giacomo Boni undertook a stratigraphic excavation to the N.W. of the foundation of *Equus Domitiani* in the middle of the Forum Romanum. The finds, unfortunately never published, are preserved in perfect order in the storeroom of the Forum Museum; and thanks to the generosity of Italian colleagues of whom I wish to mention in particular Professors P. Romanelli and S. Puglisi, this material was placed at my disposal for study and publication. When it turned out to be necessary to carry out a supplementary excavation, permission was readily granted me by the Italian authorities.

Immediately N.W. of *Equus Domitiani* I excavated an area, 5.90 m. in length and 3.40 m. in width, down to virgin soil reached at a depth of 5.87 m. below the Forum level of the Empire. When the excavation was finished there remained an earth wall in which the story of the centuries could be read in the superimposed strata.

Fig. 1 shows the section of these strata. Three categories of layers are easily distinguished. The first category comprises layers 1-19 representing successively raised pavement-levels of the Forum Romanum with earth filling below the pavement-beds on which the slabs, now missing, once rested. In the section the beds are indicated in dark shade while the fillings are in lighter colour. Layer 2 is the bed for the Forum pavement of the Empire and layer 1 is a thin adjustment stratum of earth necessitated by the fact that the covering slabs of travertine were of unequal thickness, as shown by the section. Layers 3-19 are fillings and beds for six pavements. The filling of the first pavement counted from below, i.e. layer 19, is very compact and contains numerous pieces of chipped tufa blocks, and it can be seen that an entire block had also been thrown into the filling. I shall return to an explanation of this phenomenon. The pottery found in the filling indicates that this pavement was laid c. 450 B.C. Most of the filling below the second pavement consists of burnt remains, giving evidence of the devastation caused by the Gallic invasion in 387/6 B.C. This second pavement seems to have been laid very soon after the catastrophe and may therefore be assigned to c. 385-380 B.C. Below the third pavement there was pottery of the 4th century B.C. The stratification on the Comitium has shown that the pavement there which corresponds to the third Forum pavement, may be placed in chronological connexion with the remodelling of the Comitium and the Forum Romanum undertaken by C. Maenius, and, in consequence, we may assign the third Forum pavement to c. 335 B.C. On the evidence of the potsherds found below the fourth pavement its date can be determined at c. 200 B.C. The fifth pavement was laid in the time of Sulla and the sixth pavement belongs to the years about 50 B.C. when extensive innovatory works were carried out in the Comitium and the Forum Romanum; in the former place the works were supervised by Faustus Sulla after the destruction of the Curia Hostilia by fire in 52 B.C., while the changes in the Forum Romanum are usually connected with Caesar. Thus it can be stated that the six pavements of the Forum considered above belong to the Republican period. On top of

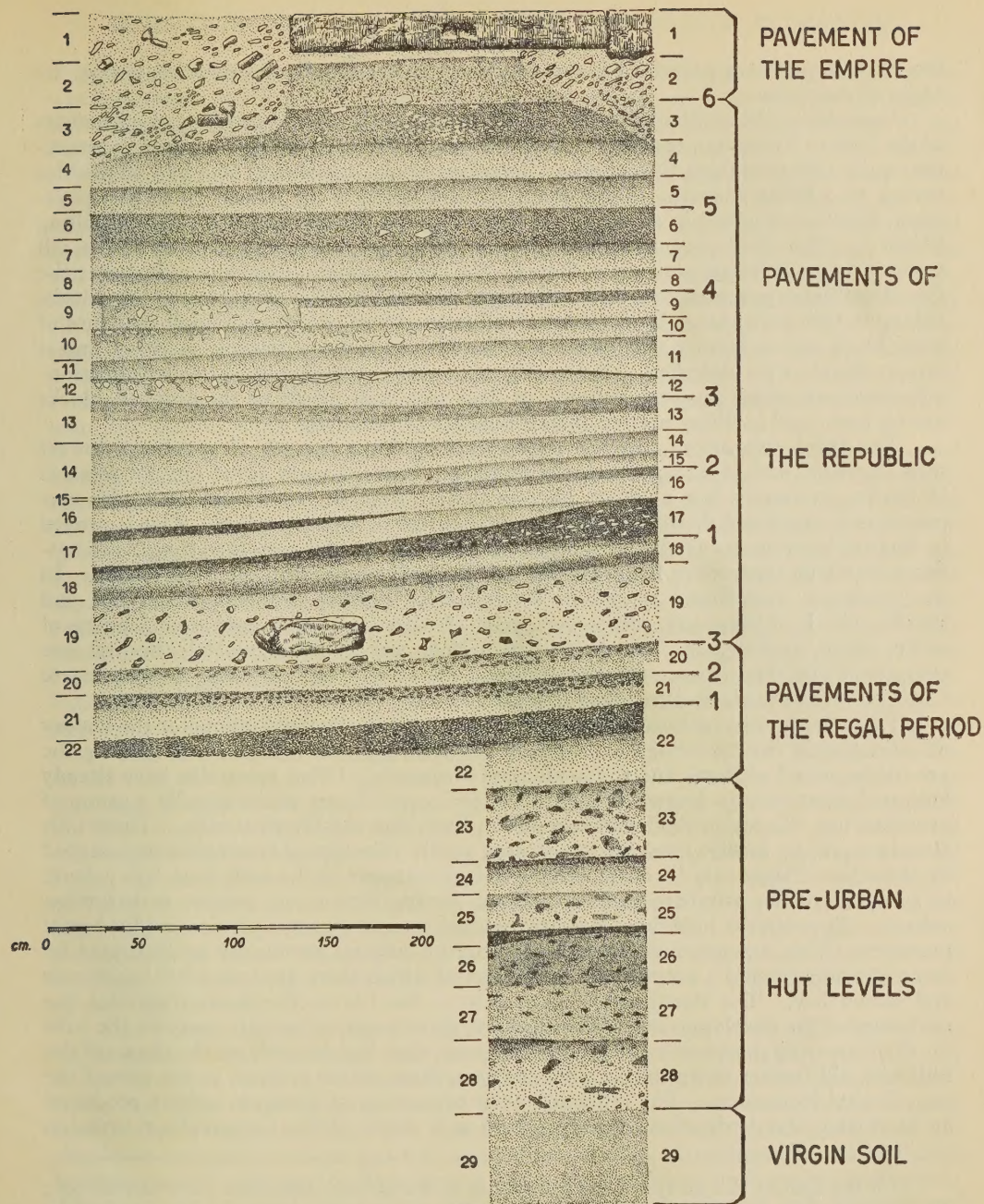


FIG. 1. SECTION OF THE STRATIGRAPHIC EXCAVATION IN THE FORUM ROMANUM

the sixth Republican pavement is the pavement of the Empire constructed during the reign of Augustus.

Continuing the study of the section we proceed to the second category of pavements of the Forum Romanum, comprising layers 20–22. These pavements are of a construction quite different from that of those described above, consisting of a bed of pebbles resting on a filling of earth. They date from the period of the Kings. The first pavement, layer 22, was laid in c. 575 B.C. as shown by the pottery found in the earth filling below it. The third pavement, layer 20, can be assigned to the beginning of the 5th century B.C. and lasted until the middle of the century when, as mentioned above, the first Republican pavement was laid. Remains of a wall built of well-dressed tufa blocks belong to this pavement. Evidently the wall formed part of a monumental structure of some kind, but no literary tradition about that structure seems to have survived, and it cannot therefore be identified. It was oriented to the cardinal points and was intentionally destroyed when the first Republican floor was laid, pieces of the chipped blocks having been used as filling material below that floor (*cf.* above).

The third category of strata is represented by layers 23–28, of which the lowest rests on virgin soil, layer 29, consisting of alluvial clay. Layers 23–28 are not remains of Forum pavements but of huts: the walls were built of reeds reinforced with clay (wattle and daub) and the roof-beams were supported by wooden poles. This is proved by finds of burnt lumps of clay with impressions of reed and by preserved remains of hut-floors in which there were holes for the wooden poles supporting the roof-beams. In the hut-layers were found numerous fragments of pottery, fire-supports, reels, and spindle-whorls of terracotta, carbonized grains of wheat and beans, and animal bones of cattle, sheep, and swine. Two habitation periods can be distinguished, the first one comprising the time from c. 650–625 B.C. and the second one extended from c. 625 to c. 575 B.C. when the huts were razed to make room for the first Forum Romanum.

This stratigraphic excavation has yielded chronologically fixed points for the history of habitation in the centre of Rome and, in addition, a primary material elucidating the pre-urban period of Rome and the foundation of the city. Other researches have already indicated what we now know: that which later became Rome was originally a group of primitive hut villages on the Palatine, Caelian, Esquiline and Quirinal hills. These hills were occupied by settlers from the 8th century B.C.¹). The type of civilization represented by these hut villages may be characterized as local variants of the early Iron Age culture in Latium—rustic, primitive and sequestered, having little or no contact with foreign culture. Between the hills there was uninhabited ground that was partly used for burial purposes. This topographical isolation favoured a cultural peculiarity as illustrated by ceramic evidence, and a political particularism, of which there are traces left in the cult and sacred law. The stratigraphic excavation in the Forum Romanum illustrates the next stage of the development. About 650 B.C. there began to be little space on the hills for the increasing population and, in consequence, they had to settle on the slopes of the hills and still further down in the valley between these, as far as down to the area of the later Forum Romanum. The influence, then beginning, of Etruscan culture produced an increasing standardization of culture, and as a result of the topographical isolation

¹ On the Caelian Hill no systematic excavations of the earliest strata have been carried out; only remains of the Empire and of the Republican period have been examined. The fact that the Caelian hill was one of the *montes* celebrating the pre-urban festival of Septimontium (*cf.* below) shows that the habitation of this hill must go back to the pre-urban period, but how far back can only be ascertained through excavation.

being broken a certain collaboration in cult and politics between the village communities was organized. In literary tradition this stage of development is reflected by the festival called Septimontium, celebrated by the inhabitants on seven hills within the area of the Palatine, Caelian and Esquiline, while the Quirinal was excluded from it. The festival was not celebrated in common by the population within this area but by the inhabitants of each hill separately, each hill having its own organization for this festival, its own *magistri* and *flamines* as shown by later inscriptions. The organization of this festival does not therefore indicate a political fusion of the communities on the Palatine, Caelian and Esquiline, but rather, at most, some sort of confederation based on connexion in cult, representing a transitory stage between the isolated village settlements and the unified city, Urbs. I must confess that I have previously been sceptical about the Septimontium stage, if I may call it so, in the pre-urban history of Rome and I have reckoned with the possibility that we had to do with a case of learned speculation. The archaeological evidence produced by the Forum excavation has, however, convinced me that the Septimontium stage is not a fiction but a concrete reality.

About 575 B.C. an epoch-making event occurred and there cannot be any doubt about its significance. The primitive hut settlements came to an end and on the top of the razed huts the first Forum Romanum was laid out. The period of the villages, that of the pre-urban villages, is gone. The earlier villages, those of the Septimontium and of the Quirinal, are united into one community, a city, and the point where they all met was occupied by a market-place, the Forum, the political and economic centre of the new city. This was the foundation of Rome. The results of the Forum excavation are confirmed by other facts. On the Capitoline extensive soundings have been made below and near the temple of Jupiter Optimus Maximus, between this temple and the Portico del Vignola, on the Piazza del Campidoglio, at the temple of Veiovis below the Palazzo Senatorio and in many places along the *Clivus Capitolinus*. Nowhere has a stratum or a deposit been found that is earlier than the second quarter of the 6th century B.C. This is a conclusive result showing that the habitation of the Capitoline is not earlier than about 575 B.C. At that time the Capitoline hill, hitherto uninhabited, was chosen as the *arx* of the united city. At the eastern boundary of the Forum, opposite the Capitoline, two buildings were erected, which—like the Capitoline—are intimately connected with the foundation of Rome both in sacred and political respects, viz. the Regia, the chancellery of the rex, the sovereign of the new city and state, and the temple of Vesta, the sacred symbol of their persistence. Both these edifices were built at the same time as the first Forum Romanum was laid out, as proved by the earliest pottery connected with them. Thus the work of political unification was accomplished: Rome had been founded. *Ab urbe condita* is the time after c. 575 B.C. That is the initial date of the Regal period.

The Forum excavation has also contributed to the solution of the final date of the Regal period. The traditional date for the introduction of the Republic, 509 B.C., is a fiction, as shown recently by Krister Hanell in his work 'Das altrömische eponyme Amt' (*Acta Inst. Rom. Regni Sueciae*, Ser. in 8°, II, 1946). A critical study of the literary tradition leads to the conclusion that the transition from the Regal period to the Republic should be assigned to about 450 B.C. and this conclusion is confirmed by the archaeological material. During the Regal period Rome was dominated by Etruscan civilization. The year 509 B.C. does not mark a change in the cultural situation and Etruscan art continued to flourish in Rome until about the middle of the 5th century B.C. At that time contact with Etruscan culture was suddenly broken. The result of the Forum excavation points in the same direction. The date of the pavement represented by

layer 20 approaches the traditional date for the introduction of the Republic, but that pavement belongs to the same structural category as the first and second pavements of the Regal period. It does not introduce a new period in the history of habitation in Rome, but represents the concluding phase of an old one, the third phase of the Regal period. A new structural type marking a break in the history of habitation is introduced by the pavement which I have called the first Republican pavement and that can, as mentioned, be assigned to the middle of the 5th century B.C. Full documentation of the opinions here presented will be given in my work 'Early Rome' to be published in the *Acta* of the Swedish Institute in Rome.

Finally I wish to summarize the evidence brought forward by the Forum excavation for the determination of the principal periods of the history of habitation in Republican Rome. I have already treated that subject in connexion with my investigation of the architectural history of the Comitium and I refer the reader to that paper, 'Il Comizio Romano dell'età repubblicana', published in *Opuscula archaeologica* II (*Acta Inst. Rom. Regni Sueciae*, v, 1941, pp. 97 ff.). The six architectural epochs of the Republic determined on the stratigraphic material from the Comitium agree with the six periods distinguished on the stratigraphic evidence from the Forum Romanum. The first Republican period comprises the time between c. 450 and 387/6 B.C. when the Gauls invaded Rome, devastating the city. After the Gallic invasion a new period begins, marked by the second Republican pavement, a period of restoration and concentration of power against external and internal dangers: the Republican city-wall and the temple of Concord erected after the enactment of the Licinian laws are monuments characteristic of the time. The victory in the Latin war in 338 B.C. inaugurated an important architectural period, its buildings in the Forum Romanum belonging to the third Republican pavement. Latium became Roman and, through the treaty with Capua, Greek civilization was allied to Rome, and the incipient influence of early Hellenism is apparent in Roman temple architecture of the 3rd century B.C. After the second Punic war Hellenistic culture penetrated into different branches of Roman life, a necessary consequence of the world-politics on which Rome embarked. The fourth Republican pavement of the Forum was one of the scenes for this cultural process. The Forum Romanum was rebuilt in monumental style: in 184 the Basilica Porcia was erected, in 179 the Basilica Aemilia, and in 170 B.C. the Basilica Sempronia. This urbanistic architecture of Rome represents an artistic synthesis of Italic individualism and Hellenistic form. After Sulla's return from the campaigns in the East this architectural process was accomplished during the time corresponding to the fifth Republican pavement of the Forum. The short period represented by the sixth and last Republican pavement is the Caesarian prologue of the Empire: in this period, or, more exactly, in 54 B.C. Caesar's forum, the Forum Julium, began to be built, inaugurating the series of Imperial fora.

What was Murrhine?

by C. N. BROMEHEAD

IN 1855 C. Müller¹ published the first volume of his *Geographi Graeci Minores*: in the introduction to the 'Periplus of the Erythraean Sea' he states that he has studied six hundred writers on the question 'what was murrhine, but that it may be now regarded as certain that murrhine vases were made of fluorspar'. Since, however, the opinions which Müller discarded have been revived by writers entitled to respectful consideration, some of whom have also given reasons for rejecting the identification with fluorspar, it seems desirable to review the matter and to put on record certain new evidence.

The first stage in the enquiry must be to decide whether murrhine, i.e., the Greek and Latin *murrha* or *morria*, was a natural substance or an artificial product. The late B. Laufer², of the Field Museum of Natural History, Chicago, has adopted the latter view and argued the case at length; he maintains that some Chinese pottery with coloured glazes, discovered by him, attributable to the Han period (say 3rd century A.D.) is murrhine ware. In his discussion he necessarily lays great stress on the line in Propertius (iv, 5, 26) 'murraque in Parthis pocula cocta focus' which he translates without comment as 'murrine goblets baked in Parthian furnaces' (p. 126); on another page he puts 'kilns' for 'furnaces', but this may be a quotation from a German rendering. Even if Propertius intended to state that the cups were artificial products from a kiln, to accept him as against the clear evidence of Pliny seems like accepting Coleridge as against, say, Herschel for the presence of a star within the horns of a crescent moon. That Pliny regarded murrhine as a stone is absolutely certain, and that he was so understood throughout classical times is confirmed by Isidorus (*Etym.* xvi, 12); in one passage (xxxiii, 2) Pliny says that from this same earth (i.e. that supplies metals) we have dug up (*effodimus*) vessels of murrhine and vases of crystal; in another (xxxv, 46) he contrasts murrhine and crystal with earthenware (*fictilia*). Laufer translates *fictilibus simpluviis* as 'plain earthenware ladles' (p. 134) and says 'the contrast intended by the author between the rustic unglazed indigenous Italic earthenware and the pretentious, glazed, imported Oriental pottery is self-evident. Pliny, further, includes emerald, gems and murrhine as the most valued products of the earth, comparable with diamonds (xxxvii, 78); Laufer writes (p. 134) 'in my opinion it [this passage] is of no consequence'. He ignores completely Seneca's classification of *murrhina pocula* as *gemmae* (*De Ben.* vii, 9).

The use of the word *focus*, however, is sufficient to show that Propertius's language is poetic: *fornax* would be more natural for a kiln, but even that is used by poets for the internal fires of Aetna. Pliny states that murrhine is supposed to be a vapour condensed underground by heat (*humorem putant sub terra calore densari*, xxxvii, 8) in contrast with rock-crystal which was ice made permanent by excess of cold. This passage is quoted

¹ Müller, C., *Geographi Graeci Minores*. Paris, 1855.

² Laufer, B., *The Beginnings of Porcelain in China*. Museum of Natural History, Chicago: Anthropological series, vol. xv, no. 2, 1917.

by Isidorus and expresses the common opinion on the origin of most minerals, an opinion which lasted throughout classical and medieval times ; only that on the origin of crystal can be denied today. Why should not the ' Parthian hearths ' refer to this theory ?

Even if there were any definite statement in a classical author that murrhine was artificial, it would be incredible and we should have to regard it as on a par with the view that crystal was a form of ice. The first murrhine vases to reach Rome were carried in Pompey's triumph in 61 B.C. The latest use recorded is that by Elagabalus, whose extravagances were so great that '*in murrinis et onychinis minxit*', a passage often quoted, though I have seen no allusion to the more telling statement that M. Aurelius sold his murrhines to pay for the war with Germany. Throughout this period murrhine vases are mentioned only as the greatest rarities, fetching fabulous prices. One of the most notable features of the early empire was the rapid growth of trade between Rome and the east. Pottery that could fetch such prices as the 300 talents given by Nero for one murrhine cup would inevitably have reached Rome in increasing quantity, though there would of course have been a corresponding fall in value. Nothing of the sort occurred. Murrhine must have been a rare mineral found only in small quantity and soon exhausted.

It seems unlikely that any modern writer would have seriously considered equating murrhine with any form of pottery, had it not been suggested by the old claim that murrhine was Chinese porcelain. This was first put forward by Cardan in the middle of the 16th century (*De Subtilitate*, IV) and was supported by Scaliger, who usually opposed him, followed also by Pancirollus, Hofmann and many others. The first reasoned opposition came from De Boodt³ (Bk. II, chap. LXXXV and XCII) who favoured Sardonyx or Onyx. The porcelain theory is now known, as Laufer himself admits, to be historically impossible, and appears to be founded on a chance analogy. In classical times vases and cups called murrhine reached Europe from the east ; they were greatly valued for the beauty and variety of the colours, were translucent and fragile. Chinese porcelain reached Europe in the 16th century, and the same words apply. ' Therefore ' the two materials were the same ! Laufer's glazed pottery preserves this long-held view with the minimum of correction and the identification with murrhine enhances the importance of his discovery.

We may now approach the second part of the problem. Assuming that murrhine was a natural mineral, what mineral was it ? Neither jade, soapstone, amber nor obsidian, all of which have been suggested, seem to call for serious consideration. The two important identifications are with agate and with fluorspar. ' Agate ' is a convenient and familiar name for various banded forms of chalcedony, a colloid form of silica, which in the crystalline state is quartz or rock-crystal. Various names have been given both in classical and in modern times according to the colours of the mineral and the arrangement of the banding—onyx, sard, sardonyx, cornelian and others.

In 1546 Agricola⁴ definitely identified the murrhine of Pliny with onyx (Bk. VI, pp. 296, 7) using, however, the word ' chalcedonius ' in his glossary of German equivalents for Latin names (p. 481). De Boodt, in 1609, under the heading Sardonyx, says ' I consider that the murrhine vases so greatly esteemed in ancient times were made of this gem and were not what is now termed porcelain, brought from China ' ; and under Onyx : ' among the Roman vases of onyx were called murrhine '. One argument on the

³ de Boodt (Boethius), A., *Gemmarum et Lapidum Historia* (1st ed., 1609) ; 3rd ed. Leyden, 1647.

⁴ Agricola, G. (=Georg Bauer). *De Natura Fossilium Libri Decem*. Basle, 1546.

other side is given by Hofmann,⁵ *s.v.* Myrrha—if the murrhine vases of the ancients were made of onyx, why were they called murrhine, or what reason can be given for this appellation, and why are murrhines everywhere distinguished from onyx ? He favours porcelain, but adds the cautious statement that neither the ancient authors nor their commentators knew what murrhine was. The various forms of chalcedony and agate had, of course, been familiar to the Greeks and Romans for centuries. Achates and onyx are given by Theophrastus and, with sard, in the Septuagint ; many other varieties are mentioned by later writers. Pliny describes many colour-varieties, moss-agate, etc., mostly under compound names such as dendrachates for the last. It is certain that, had murrhine been any form of agate or chalcedony, he would have recognised and described it as such.

The greatest authority to favour agate was Rev. C. W. King,⁶ and it was probably his influence that led Professor K. C. Bailey to accept this identification and the new Liddell and Scott to give ' perhaps agate ' under the Greek *μόρρη* (used by Pausanias) and the forms of *μορρίνη* in the Periplus of the Red Sea. It is strange that King himself should state very clearly, in favour of this theory, one of the strongest arguments against it (p. 182) :—' Fragments of bowls made of Agate (but of no other irregularly coloured stone) are turned up in abundance in the soil of the ancient capital, and often of a radius that bespeaks the extraordinary circumference of the perfect vessel. Such pieces, if not large enough to be preserved as antiques, are cut up into brooch-stones and every spring furnishes the Roman lapidaries with an inexhaustible supply. Perfect vessels . . . are rare, yet a comparatively large number are yet in existence '. An ' inexhaustible supply ' of fragments of murrhine is a flat contradiction of every classical allusion to the substance without exception. The identification with agate is tentatively accepted by Bailey⁷ (Pt. I, p. 176), who writes ' it is doubtful whether the identity of this substance will ever be certainly known. The three main theories are (a) that it was porcelain, (b) that it was fluorspar, (c) that it was some variety of silica, such as onyx, opal, or agate, probably the latter. The first theory . . . may be unhesitatingly rejected . . . on the whole the opinion of C. W. King and Marquardt that agate is the substance in question is probably correct '. Bailey's objections to fluorspar are dealt with below.

It is, moreover, impossible that agate vessels should have ever approached the play of colours ascribed by Pliny to murrhine—purple, white, a fiery red, etc. The modern practice of colouring agate artificially may, perhaps, mislead classical scholars : it is true that, in the Bombay district, the natural colours of agates have been deepened by roasting at least since the 1st century A.D. ; but these are only white, yellow, brown and reddish ; blue agates are produced by a modern chemical process.

A magnificent set of eight agate vases, from the description and illustrations probably of roasted Indian agate, were found near Qift in the eastern Egyptian desert and published by Englebach.⁸ They had probably entered Egypt at Berenike or Myos Hormos. These are mentioned by A. Lucas in the last edition of his *Ancient Egyptian materials* (p. 443), who adds ' these probably came from India and are " murrhine vessels " ', such as Pliny

⁵ Hofmann, J. J., *Lexicon Universale . . . metallorum, lapidum, gemmarum nomina, naturas, vires*. Leyden, 1698.

⁶ King, C. W., *Natural History of Gems or Decorative Stones*. London, 1867.

⁷ Bailey, K. C., *The Elder Pliny's Chapters on Chemical Subjects*. London, vol. 1, 1929 ; vol. 2, 1932.

⁸ Englebach, R., Recent acquisitions in the Cairo Museum. *Ann. des Serv. des Antiquités de l'Égypte*. Tom. XXXI, 1931, pp. 126-31 and plate.

describes. At a late date agate and onyx beads were imitated in glass'. We learn from the *Periplus* (Para. 6) that glass imitations of murrhine (μορρίνη λιθία as opposed to μόρρα) were made at Diospolis, probably Thebes, and sent to the Red Sea ports, apparently for export to the east. The manufacture of glass to imitate vessels of murrhine, hyacinthus and sapphirus (sapphire and lapis lazuli) is mentioned by Pliny (xxxvi, 198). First century glass in imitation of agate is by no means uncommon and specimens that any mineralogist would regard as imitations of fluorspar by no means unknown.

The identification of murrhine with fluorspar was first suggested by F. Corsi⁹ in 1825 in the catalogue of his collection of ancient marbles and stones. In his book published in 1845 he gives as his main reason, that this mineral alone agrees with Pliny's description, and then argues the case at length, referring to every classical allusion and to the chief commentators (pp. 166-95). The same conclusion was stated, apparently independently, by F. Thiersch. It has already been mentioned that C. Müller accepted this view and regarded the murrhine question as closed.

It therefore remains to answer the objections put forward since that date and to add certain new scraps of relevant information. As far as I am aware none of the objectors has any claim to be a mineralogist, surely a necessary qualification for the subject. One objection is raised by Bailey (II, p. 176) and, less clearly, by Laufer:—'The greatest objection to this identification is the fact that no mention is made of the crystalline form of fluorspar. The least observant, on inspecting a collection of fluorspar, must be struck by the very perfect cubes in which the substance almost always crystallizes. The form is so simple and striking that it is hard to believe that Pliny, while describing accurately the form of rock-crystal (xxxvii, 26) would have omitted altogether to describe that of fluorspar'. It is true that specimens of fluorspar exhibited in a museum usually show the cubical crystals very clearly—they are selected for that purpose. But Pliny never saw the natural mineral, only vessels carved from it. For this purpose the massive variety, with colour-banding, would necessarily be used. A collection of wrought fluorspar, whether Derbyshire Blue-John vases, German ornaments, or Chinese vessels and figures, would scarcely ever reveal the cubic structure.

Another point arises from Pliny's statement (xxxvii, 7) that 'a person of consular rank, who some years ago used to drink out of this cup, got so passionately fond of it as to gnaw its edges, an injury, however, which has only tended to enhance its value'. Laufer seems to think that it would be impossible to gnaw a cup cut from 'agate, fluorspar, or any other stone with which these vessels have thoughtlessly been identified. Agate, which has a hardness of about 7 degrees and, having no cleavage, is tough, would certainly need to be very thin: yet many children have bitten pieces out of glass tumblers. Fluorspar, on the contrary, has a hardness of only 4 degrees, slightly less than the enamel of a human tooth, a cleavage which is mineralogically described as perfect, and is brittle: it is very easy to nibble little pieces off the rim of a fluorspar vase (*experto crede*!). In fact Pliny's use of the word *fragilitas* (xxxiii, 2) the gnawing of the cup just mentioned, and the many allusions to the brittleness of murrhine by other authors, and again the reflections of rainbow colours near the edges (xxxvii, 8), due to incipient cracks along cleavage planes, all favour fluorspar.

The massive mineral is translucent, never transparent, and may in parts be almost opaque. This property is referred to by Martial (iv, 85)—it would be quite impossible to distinguish two different wines when seen through a fluorspar cup—and may be

⁹ Corsi, F., *Delle Pietre Antiche Trattato*. Rome, 1845.

the meaning of Pliny's phrase '*Splendor his sine viribus, nitorque verius quam splendor*' (xxxvii, 8).

Mention is also made of a pleasant odour from murrhine cups used for drinking wine. Laufer considers that this is intelligible only if the question is of pottery, 'scented minerals or glass are not conceivable': yet in a footnote on the same page (131) he refers to pottery which derives its scent from the mineral, a 'reddish odoriferous clay' from which it is manufactured. Carved fluorspar may well have a slight odour or impart a faint taste to wine, though this is not due to a natural property. In the process of cutting and polishing Derbyshire fluorspar, it was the custom to soak the mineral repeatedly in hot rosin in order to overcome, as far as possible, the tendency to fly to pieces. Traces of this artificially added odoriferous material can sometimes be detected now in vases manufactured a hundred years ago.

A passage to which I have not seen any reference made by any commentator on this subject is in the *Silvae* of Statius (iii, 57) :—'cups of crystal and of weighty murrhine' (*murrasque graves*). Rock crystal has a specific gravity of 2.65, fluorspar of 3 to 3.25, so that the latter would be noticeably heavier than a crystal cup of the same bulk. Laufer does not give the specific gravity of the Han pottery which he claims as murrhine, but he describes the body of the ware as a 'porous porcelain froth' (p. 88), presumably much lighter than crystal.

Pliny states that murrhine vessels come to us from the east; they are found at many localities, not (otherwise) noteworthy, in the kingdom of Parthia, more precisely in Carmania. Many occurrences of fluorspar have been reported in and around this region by geologists, especially those of the Anglo-Iranian Oil Company; one personal correspondent unconsciously uses almost exactly Pliny's words to describe the colours.

Laufer (note, p. 129) says 'it is astonishing with what high degree of tenacity the unfounded opinion of fluorspar vessels could hold its position in the face of the bare fact that no such vessels ever existed in ancient Persia, Egypt, or in classical antiquity and have never (*sic*) come to light . . .' In the *Century Dictionary* it is justly remarked under *Murra*, 'the principal objection to this theory is that no fragments of fluorspar vases have been found in Rome or its vicinity'. This statement is definitely false. Corsi mentions two antique fluorspar vases found in Rome, one in the Museo Kircheriano, which he says 'so completely answered the description of the *murrhine* that it seems as if it had been in the hands of Pliny when he wrote his description of that material: another in the possession of Signor Gillet-Laumont'. I have quoted this passage from Westropp (pp. 181, 2), not direct from Corsi, to show that the facts are available in English as well as in Italian. Middleton¹⁰ describes the Kircheriano specimen as 'a small shallow cup made of fluorspar, which appears to be ancient' (p. 17): he also says that 'a few other pieces [of fluorspar] have been found in Rome'. Westropp¹¹ also gives a full account of eight blocks of fluorspar 'lately discovered at Rome, at the Marmorata, by Signor Visconti'. These probably came from Regensburg on the Danube and are irrelevant to the murrhine question.

It is not only at Rome, however, that vases of fluorspar have been reported. It would be reasonable to expect that some of the original murrhine vessels were in Pompeii and Herculaneum at their destruction in A.D. 79. It is stated by Egglestone that 'in excavating at the ruins of Pompeii two large "Blue John" vases of excellent workmanship were found'; but this tale has been officially denied: if these vases ever existed, they

¹⁰ Middleton, J. H., *Ancient Rome in 1888*. Edinburgh, 1888.

¹¹ Westropp, H. M., *Manual of Precious Stones and Antique Gems*. London, 1874.

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disappeared, perhaps by doubtful means, into a private collection soon after the reported discovery, which cannot now be verified.

In 1940 the late T. Sheppard published statements that on a visit to Naples he had seen two vases recently found at Herculaneum, which he identified as made of Derbyshire Blue John. While quite familiar with the Derbyshire material, Sheppard had little or no knowledge of continental or oriental fluorspar. If they have survived the war, these vases should be available for expert examination.

This paper was first put together to show that murrhine *must have been* fluorspar. Since then two vases of carved fluorspar have been described and illustrated by Loewental and Harden.¹² They are of the 1st century A.D., found near the Turco-Syrian border, one at any rate in circumstances that prove that it was a highly valued treasure. As indicated by the title of their paper it is beyond question that they were 'Vasa Murrina'. In short, murrhine *was* fluorspar.

¹² Loewental, A. I. and D. B. Harden. 'Vasa Murrina'. *Journ. Roman Studies*, vol. xxxix, 1949, pp. 31-7. Plates v-vii.

Inyanga: a preliminary report

by ROGER SUMMERS

INYANGA is a district in Southern Rhodesia whose monuments were brought to the notice of readers of *ANTIQUITY* a short while ago¹. A Fund raised by private subscription mainly from Rhodesian sources has recently sponsored excavations in this district and at the conclusion of the second season's work it seems desirable to make a preliminary report. The excavations were directed by the writer who was fortunate in having the very skilful assistance of the Colony's Inspector of Monuments, Mr K. R. Robinson. Unfortunately no amateurs were able to assist us.

The only serious work done previously in this district was that undertaken by the late Dr Randall MacIver in 1905². At that time Rhodesian archaeology was an untouched field and MacIver had no comparative material to work on, so that it was high time that something should be done to put Inyanga into its place beside other cultures of the Rhodesian Iron Age known to us through the efforts of subsequent workers, notably Miss Caton-Thompson³. As the area covered by the Inyanga Ruins is between 2,000 and 3,000 square miles it was rather a problem to know where to start in order to get the greatest value out of our somewhat slender resources. It may be thought that in so vast an area we should have made use of aircraft or at least air-photographs in our preliminary reconnaissance. For several reasons, limited resources not the least, we decided not to use aircraft at first but hoped to have air-photographs for study later. Through the good offices of the Surveyor-General of Southern Rhodesia we are likely to have the use of air photos of part of the area very shortly. When they have been examined further work may be suggested.

Preliminary ground reconnaissances were made in 1949 and early in 1950 from which it was clear that we had to deal with at least two cultures and probably more. Two of these seemed to be present at the *van Niekerk Ruins* where MacIver had worked, and there seemed a possibility of a third culture on the uplands around the village of Inyanga proper. During our reconnaissance we heard of, but did not visit, extensive ruins many miles north of Inyanga. We planned therefore to spend our first season at van Niekerk Ruins where we hoped to establish a cultural succession which would help us to unravel the problems of the lesser known ruins further north. We planned, too, to spend some time in the Inyanga upland Zone excavating in typical ruin groups there.

The areas in which we proposed to work varied in altitude between 3,000 and nearly 7,000 feet, and as the danger of malaria makes it impossible to camp during the wet season (November to April) it was somewhat difficult to arrange a programme which would enable us to escape the extreme heat of October in the low country and the bitterly cold winter of the uplands. Fortunately a knowledge of the country enabled us to overcome these difficulties and avoid any loss of time or efficiency from bad weather or sickness. From the beginning of June to the end of August 1950 we were continuously employed at van Niekerk Ruins and excavated on twenty different sites besides making

¹ Crawford, O. G. S., 'Rhodesian Cultivation Terraces', *ANTIQUITY*, XXIV, 96-9.

² MacIver, D. Randall, *Medieval Rhodesia*. London, 1906.

³ Caton-Thompson, G., *The Zimbabwe Culture*. Oxford, 1931.

surface collections. For part of July and August 1951 we worked over a very large area in the northern part of the district, excavating at thirteen sites and making very extensive surface collections; finally, most of September was spent in the Inyanga Uplands where three sites only were excavated, although extensive reconnaissances were made.

Van Niekerk Ruins proved rather a disappointment. Occupational material was extremely sparse, and six long weeks passed before a midden associated with the Ruins was located; this is totally different from the case of ruins of the Zimbabwe Culture as students of Miss Caton-Thompson's work will know⁴. However, details of the construction were studied in a large number of cases, and for the first time in archaeological work in Rhodesia (and, I think, in all Southern Africa) a study was made of carbonised grain found in sealed deposits.

PRE-RUIN CULTURES. Despite our lack of finds in the ruins proper we were rewarded by finding traces of two pre-Ruin Iron Age Cultures: one of these comes from MacIver's 'Place of Offerings' and the other from somewhat similar deposits a mile or two away. The pottery associated with both these cultures belongs to the very widespread class which I have described elsewhere as 'Stamped Ware'⁵ and the Cultures may therefore be assigned to Rhodesian Iron Age A. Although the ceramic differences are slight they are unmistakable when large assemblages of decorated sherds are compared, but the main difference is in the associated finds. The 'Place of Offerings' ware (which I have called *Ziwa 1*, from the name of the mountain at whose foot MacIver dug) is associated with copper and iron objects, shell beads, cowrie shell and worked bone, whilst the other ware (*Ziwa 2*) although also associated with copper and iron has none of the other objects but does have as its associates glass beads—pale blue transparent canes with sharply snapped ends.

Although the stratigraphical evidence for the relative age of *Ziwa 1* was not impeccable at Niekerk it seemed probable that it was older than the Ruins. For the age of *Ziwa 2* we obtained good evidence in that finds belonging to this Culture lay below huts on whose floors were sherds and other finds common in Ruin assemblages. This stratified site is on the slope of *Ziwa* mountain, a granite peak towering a thousand feet or more above the Ruins, and seemed to be associated with very rough granite-faced terraces quite unlike any which were associated with the Ruins; we could not however prove the association on this site.

During 1951 however we were fortunate in finding these same Iron Age A cultures further north, and were able to prove to our own satisfaction that *Ziwa 1* was indeed older than the Ruin terraces (probably much older, for a thick deposit of sterile hill wash, eroded and redeposited, lay between the terrace level and the underlying *Ziwa 1* midden). Moreover, a *Ziwa 2* deposit lying on, and to some extent in, rough granite faced terraces on the slope of a granite mountain was located several miles from the original *Ziwa* site.

There is no stratigraphical evidence for the relative age of *Ziwa 1* and *Ziwa 2*. I have suggested the order given purely on the absence or presence of imported glass beads—a doubtful criterion and one which is liable to be upset by future work; however the two cultures are sufficiently unlike to need differentiation, and accordingly I have made a guess at their order.

THE RUINS. Mr Crawford has already pointed out the probability that these ruins were in the main cultivation terraces⁶ and MacIver, although wedded to the idea

⁴ Caton-Thompson, G., *The Zimbabwe Culture*, Oxford, 1931.

⁵ Summers, R., 'Iron Age Cultures in Southern Rhodesia', *S. Afr. Jnl. Sci.*, XLVI, 95-107.

⁶ Crawford, O. G. S., 'Rhodesian Cultivation Terraces', *ANTIQUITY*, XXIV, 96-9.

of fortification in his first book, later accepted the generally held theory of cultivation.⁷ We were able to demonstrate the validity of this theory in three ways.

In the first place all the terraces connected with the ruin complex at van Niekerk are on dolerite, a rock whose decomposition gives rise to a fertile soil very suitable for grains of the millet family. Secondly many of the enclosures at van Niekerk Ruins are partly or wholly taken up with structures too small for huts and bearing a close similarity to modern grain stores. Finally, we found in deposits connected with presumed grain stores or with querns, carbonised seeds which have been identified by Dr H. Wild, of the Southern Rhodesian Division of Botany, as millets, sorghums and pulses. Dr Wild spent some time at our camp and greatly widened the scope of our work by directing our attention to various botanical points in connection with the terraces. He is of the opinion that the grain would not have grown in the often waterlogged meadows or *vleis* between the terraced hills but that on the well-drained terraces a good crop could have been reaped for several years.

The walling both of the terraces and of the buildings connected with them looks at first sight rough and unfinished, but further acquaintance with it reveals its finer points and leaves one in no doubt about the skill of builders who with the greatest economy of labour regularly used boulders weighing anything up to a ton for their building. There is no doubt that, before the van Niekerk Ruins were terraced, the dolerite hillsides were strewn with boulders of all sizes; indeed we once had the unpleasant task of climbing one such slope during a reconnaissance, and it is certain that, in order to grow anything at all, a general tidying up would have had to be undertaken. The people of the Ruins seem to have been almost unnaturally tidy, for there are little heaps of stones piled up not only in cultivated lands—a common sight anywhere in the world—but also round the base of baobab trees, round immovable boulders and at the foot of terraced hills. Such tidiness is no longer a local African trait and one is bound to suggest that it was only extreme need which led the Ruin dwellers to go to such lengths.

A further indication of the difficult conditions under which they lived is the fact that certain kopjes which contained remains of grain stores were quite clearly strongholds, completely walled and with entrances overlooked by small rooms very suitable for a guard. It seems therefore that the more important stores of grain had to be kept under military guard.

Many of the enclosures contained pits about 15 feet in diameter entered by an inclined passage in the sides of which were two holes, one as much as 10 feet deep into which a wooden bolt could be withdrawn and one shallow to form a staple into which the bolt would fit. Occasionally there is provision for locking the bolt into position by inserting a peg at its inner end. This simple bolt device was found in many places in these Ruins but is unknown elsewhere in Southern Rhodesia. In nearly every case the wooden bolt has disappeared but in one building we found one still in its original position.

Very little dating evidence was found in the Ruins but from a few beads from some four different sites it seemed probable that an 18th century date is not unreasonable. Ring counts from a number of trees of common species now growing on the terraces gave an average age of about 200 years thus confirming the bead evidence. Despite the age of the trees they are very small timber and the present inhabitants (Waunyama, a Shona tribe) have great difficulty in getting enough for fuel and building. No doubt the stone buildings were in part the response of former people to a relatively treeless environment.

⁷ MacIver, D. Randall, 'Rhodesia (note on Archaeology)', *ANTIQUITY*, I, 103.

In 1951 we examined a very similar group of ruins further north. These showed a number of minor differences in planning, building technique and pottery but in general the cultural picture seemed very similar to that at Niekerk. Dating evidence was just as scanty but again we recovered, at the expense of a great deal of sieving, just enough beads to convince us that both sets of ruins were contemporary. These northern ruins were probably the first to attract European attention; a newspaper article on them appeared in 1898, and Carl Peters, a German traveller, described them in 1901, garnishing his description with the most fantastic speculations⁸.

The more northerly ruins were much less encumbered with bush than those at Niekerk and we were able to study their construction more fully. Thus we confirmed our previous impression that hillsides had been terraced bit by bit apparently in accordance with the dictates of shifting agriculture, so that what one sees to-day are the imperishable remains of a century or more of continuous work, only a very small proportion of the whole being cultivated at a time. The sparseness of occupational debris is itself evidence of constant removal to new fields whilst the very ubiquity of terracing in this district shows how thoroughly the past inhabitants sought out every scrap of available arable land.

The ruins are all in lowland country between 3,000 and 4,000 feet.

THE INYANGA UPLANDS. At the end of the 1951 season we spent some time on the Inyanga Uplands—5,500 feet or higher. Here we were in an area of cold winds and almost perennial rain, totally different from the warm dry lowlands which, though only a few miles away, are in the rain-shadow of the mountains of the Inyangani group (Inyangani, 8,517 feet). Not unnaturally the cultural pattern is different. Terraces are fewer and usually on more level ground; enclosures become circular platforms round a pit some 20 feet or more in diameter entered by a covered passage opening from outside the platform; there are irrigation channels and strongly built places perched on hill-tops which ever since MacIver's time have been called 'forts'.

Despite the short time we spent here we were exceptionally fortunate in our finds: Mr Robinson located a very extensive midden in the principal 'fort', and the pits, which had hitherto yielded nothing to various amateur diggers, provided us with a quantity of beads and information about their construction. We were not a little surprised to find in this inhospitable region beads of the series called by Beck the Zimbabwe Bed Rock Series⁹ and by Schofield Series 2¹⁰; moreover these beads were all in sealed deposits and provided unimpeachable evidence for the age of this culture—17th century according to Schofield but in Beck's view possibly considerably earlier. Apart from a few sherds in the 'fort' we found no ceramic evidence to connect these ruins with those of the Zimbabwe Culture further westwards, but there were a great many sherds directly comparable with ware made by the Manyika tribe who live in these mountains to-day.

The stone-lined pits of Inyanga are commonly called by the European population 'slave pits' because a popular interpreter of these ruins long ago suggested that the terraces could only have been built by slave labour. The natives have always stuck to the story that cattle were kept here and we see no reason to disbelieve them, with the proviso that the 'cattle' might possibly have been pigs, goats or sheep as well as very

⁸ Peters, C., *The Eldorado of the Ancients*. London, 1902.

⁹ Caton-Thompson, G., *The Zimbabwe Culture*. Oxford, 1931.

¹⁰ Schofield, J. F., 'A preliminary study of the prehistoric beads of the Northern Transvaal and Natal', *Trans. Roy. Soc. S. Africa*, xxvi, 341-72.

small oxen (the average size of the entrance passages is only 4 feet high by 2 feet wide). Hut floors and the position of refuse show that huts were on the top of the platform surrounding the pit—indeed Arkell's photograph of the pigsty house in the Sudan in a recent number of *ANTIQUITY*¹¹ is in close accord with our idea of an Inyanga stone-lined pit in its prime.

In order to draw a distinction between the various Ruin Cultures or sub-cultures at Inyanga I have suggested *Niekerk* for that of the van Niekerk Ruins and the allied culture further north, and *Inyanga Upland* is suggested for that in the Upland Zone of Inyanga proper. Before much work had been done at Inyanga I suggested lumping all the Inyanga Ruin cultures together as Iron Age C¹²; two full seasons' work does not shake my belief that here in the north east of Southern Rhodesia we have a stone building culture differing in many respects from that of Zimbabwe in the south west of the Colony, although as has been noted previously there is evidence for some connection between the two cultures.

Perhaps the most interesting historical result of our work is the identity of the Inyanga Upland culture with the modern Manyika tribe; there can be no doubt that these are the people of Manhiqua referred to by the Portuguese in the 16th century as having much gold in their land¹³. Nearly every tribe and most dynastic names in this part of the country can be identified from the 16th century Portuguese records, and our finds tend to prove, as has already been suggested by a former Native Commissioner, Mr W. H. Stead, that in this part of Southern Africa there has been no appreciable tribal movement for the last four or five centuries¹⁴.

Much more remains to be done but our work is beginning to give substance to the hitherto shadowy 'Empire of Monomotapa'.

Although the ruins took most of our time Mr Robinson managed to undertake three digs in rock shelters and to make important surface collections of stone implements. These bid fair to compel the amendment of some of our views on the Stone Age in Southern Rhodesia, which is at present known only from sites in the west of the Colony.

¹¹ Arkell, A. J., 'The Pigsty House in the Nuba Mountain', *ANTIQUITY*, XXIV, 101.

¹² Summers, R., 'Iron Age Cultures in Southern Rhodesia', *S. Afr. Jnl. Sci.*, XLVI, 95-107.

¹³ Axelson, E., *South East Africa, 1488-1530*. London, 1940. (Penhalonga, in the heart of the Manyika country is a rich gold area to-day and on the other side of the border alluvial gold is still worked by this same tribe).

¹⁴ Stead, W. H., 'The People of Early Rhodesia', *Trans. Rhodesia Sci. Assn.*, XLII, 75-83.

Sutton Hoo—a rejoinder

by R. L. S. BRUCE-MITFORD

With a note on the coins by JOHN ALLAN

DR GORDON WARD¹ says that my paper 'Sutton Hoo—Recent Theories'² is described by me as 'embodying the official views about Sutton Hoo' and 'thus claims very particular authority'. What I said in fact was (op. cit., p. 1) that, since opposite opinions had recently been expressed on certain critical aspects of the find (Pagan or Christian? Grave or Cenotaph? English or Swedish?), 'it is desirable that an official assessment of the issues should be offered to students, even though it cannot be regarded as final'. Anyone who can get into print can say what he likes about Sutton Hoo. Those officially engaged on the study and definition of the material naturally bear a greater responsibility for their utterances. They should also be acquainted with the facts, which not all commentators have been. By 'official assessment' I meant no more than that, as a sympathetic reader must at once have seen.

There is no point in arguing about the spoons, or other factors, as favouring Redwald as the man for whom the monument was raised, if the coin evidence indisputably shows that it is too late to be Redwald's. Let us then turn straight to the coins. I fully agree with Mr Crawford that 'there still has been no full and frank statement of the evidence', nor will the present 'rejoinder' supply that deficiency. I had, however, taken pains to look critically into this all-important factor, *viz.* the evidence of the coins for the date of the burial, before committing myself to the long article quoted above, and can bring forward three recent and hitherto unpublished numismatic opinions. Two of these were available to me when I wrote the article referred to, the third has since been added by Mr Philip Grierson.

First, however, I must say that Dr Gordon Ward's remarks on the coins are not helpful. He only serves up the familiar comments of 1939-40, which I personally have read and digested many times in the attempt to get to the bottom of this vexatious and all-important problem. When he fails to make any mention of Dr John Allan's post-war verdict and says (p. 13) 'no single numismatist has come forward to say that the burial cannot date between 600 and 640', it is clear that he has either forgotten, or not read, the *British Museum's Provisional Guide* (1947). He also overlooks specific statements on p. 26 of my paper, and on p. 23, e.g. '650, the earliest date considered on numismatic opinion to be possible for the Sutton Hoo ship-burial'. He ought not to assume that I made this up. What are in fact the numismatists' opinions, not of 1939, but of 1950 and 1951? The first account which I can cite is that of Dr John Allan, formerly Keeper of Coins and Medals in the British Museum. It was sent to me in 1951 and supersedes the opinions which he expressed in his lecture to the Society of Antiquaries in January 1946, and which were utilized in the Museum's *Provisional*

¹ *ANTIQUITY*, XXVI, 1952, 9.

² *Proceedings of the Suffolk Institute of Archaeology and Natural History*, vol. 25, part I, 1949, published 1950, 1-78.

Guide (p. 42). A detailed description of the Sutton Hoo coins by Dr Allan is expected shortly in the *Numismatic Chronicle*. His interim report runs as follows :

* * * *

'The Sutton Hoo Treasure contained 37 gold coins, 3 similar coins obliterated preparatory to melting down, and two small gold ingots. The coins are all of the Merovingian period of France. The ingots are of the same period, as we know that the Merovingian tax-collectors had the coins paid to them melted down, refined, and paid into the Treasury in ingots of pure gold. The Merovingian coinage began in the 6th century by copying the contemporary Byzantine coinage of Justinian, Justin II, and especially Maurice Tiberius (582-602); a clue to the chronology of the coins is given by the gradual corruptions of the types and legends and in particular the change of the reverse type from Victory to cross on globe or steps which took place towards the end of the 6th century. The Merovingian kings issued coins in their own name from the time of Theodebert I (534-48), but by the end of the 6th century the coinage of moneyers had begun which in the 7th formed the overwhelming proportion of the issues. These moneyers struck coins bearing a rude portrait of the king, but the legend consisted of the moneyer's name and place of issue. Of the latter hundreds are known from all over the country.

'More attention has been paid by students to the identification of the mints than to the chronological arrangement of the coinage. The broad outline of the latter is known from the evolution and degeneration of the portraiture and the gradual change in reverse type from Victory to cross, to cross with XP symbol, cross with omega, anchor-cross to plain cross. We have a few aids to date styles, as for example from the coins of the Church of St. Pierre de Corbie, which we know cannot be earlier than 657 when it was founded.

'All the coins from Sutton Hoo are tremisses (thirds of solidi). The following mints are represented:—Andernach, Angers, Bordeaux, Chambly, Châtre, Dinant, Gevaudan, Huy, Laon, La Trémouille, Marseilles, Metz, Mouzon, Montignac, Paris, Rodez, Sion, Sens, Troyes, Valence, Usson and Uzès.

'The find contains no coins with reverse type Victory, so that one can say roughly that there are no coins of the 6th century in it. The absence of solidi points in the same direction, for they went out of fashion in the 7th century. The only coin bearing a king's name is one of Theodebert II (595-612), an unpublished piece and apparently the second known coin of this ruler. The cross on globe reverse makes an attribution to Theodebert I impossible. Confirmation of the attribution is found in its resemblance to a coin of the moneyer Manileobo of Mouzon, who is the moneyer (at Clermont Ferrand) of the known coin of Theodebert II. The earliest coin is the imitation of Justinian of Uzès which still bears the cross potent and might even be of the end of the 6th century. The three coins with traces of the legend of Maurice Tiberius are of the early 7th century, and most of the coins of good workmanship with cross-on-globe or cross-on-steps reverse belong to the first quarter of the century. One certain date in the Merovingian coinage is given by the moneyer Eligius (who is known to be St. Eloi who died in 659) who worked for Chlotar II (584-629), Dagobert I (629-39) and Clovis II (639-57). There are no coins in the find bearing Eligius' name, but a number have types introduced by him or are later imitations of his types. To his period belong the coins of Paris with R on the cross, and the coins with omega on the cross which he introduced in the reign of Dagobert I, while the coin of Bordeaux has a portrait based on that of Clovis II.

' In the provincial mints and later issues which copied the coinage of Eligius the omega on the cross was misunderstood or forgotten and became a curve which from its resemblance to an anchor is called by French numismatists the "croix ancrée." The coins of Laon, Châtre and Chambly, for example, must be put after the direct influence of Eligius had gone, i.e. later than 660. Later still is the coin of Usson (the portrait on which is approaching in style that of Childebert III) with a reverse arrangement which became regular in the 8th century and is only found on coins known to be late, e.g. at St. Pierre de Corbie. The busts on the coins of Chambly and Montignac must be put long after Clovis II on whom they are modelled—indeed, French numismatists put such barbarous pieces towards the end of the 7th century. The two coins of Gevaudan are clearly very late when one compares them with the regular issues of this mint. The bust on the coin of Rodez is likewise later, while the small pieces with barbarous types cannot be contemporary with any coins of good style. It seems impossible on grounds of style and type to put the date of composition of the hoard before 675.

' Such comparison as is possible with other hoards points in the same direction. The Crondall hoard is dated 660–70; it was similar in constitution to Sutton Hoo but contained one coin earlier than any in the latter, while the latter has a number of barbarous pieces later than any at Crondall.

' The great Bordeaux hoard had no coins as early as some in Sutton Hoo, but the general run of types was the same and several coins were common to both hoards, as for example that of Usson, which we already believe to be late; it contains a number of coins of Gevaudan, but none as barbarous as in Sutton Hoo. It contains a number of Visigothic coins made from 576 to 680, and its date of burial is therefore known to be not before the reign of Wamba, A.D. 672–80.

' Little comparison can be made with the Wieuwerd find, which is also rich in jewellery. The latest datable coins in it were of Heraclius (d. 640); it contained a large proportion of solidi and coins of Chlotar II (584–629). A comparison would suggest that the Sutton Hoo hoard, which contained no solidi and none of the comparatively common coins of Chlotar II, is later than the middle of the century, when the Wieuwerd hoard may have been buried.

' The evidence then suggests a date not earlier than 675 for the composition of the hoard. It should be remembered, however, that this is not an ordinary hoard buried in a time of unrest and reflecting the currency at the moment of burial. It does not represent the currency of England at the time nor is it money brought back from a trip to the continent. The presence of the ingots and defaced coins in it which would not get into ordinary currency suggests it was originally in a moneyer's or tax collector's office (they were closely connected under the Merovingians) and looted from there and brought to England. How long before this took place before the coins came to be buried with the owner's other heirlooms it is impossible to conjecture'.

* * * *

Dr Allan thus places the *burial* of the hoard (i.e. the date of the grave) some time after A.D. 675; in correspondence with the writer he suggests the date c. 680.

We may now turn to the second numismatist's opinion, that of M. Jean Lafaurie, M. Le Gentilhomme's successor at the Cabinet des Médailles (where the great bulk of comparative material is concentrated) and one of the most distinguished younger French numismatists. Photographs and (by courtesy of the Keeper of Coins and Medals) a set of casts of the Sutton Hoo coins was sent to him with a brief *questionnaire*. The writer also had the opportunity of discussing the coins with him personally in Paris. The relevant part of his final reply is as follows:—

' Je viens d'examiner à nouveau les photographies et les moulages des monnaies composant la trouvaille. J'ai essayé à nouveau quelques identifications en particulier pour les triens no. 24 à 37, sans succès d'ailleurs.³ Quant à la date d'enfouissement il n'est guère possible de la placer antérieurement à 650 et certains types sont assez tardifs ; par exemple le trien no. 32 de la photographie, d'un atelier de la région parisienne indéterminé, porte "l'appendice perlé" détail que n'apparaît qu'en 650±⁴ ; les no. 28, 33, 34, 35, 36, 37 sont encore plus tardifs et doivent être en or pâle. Le numéro 10 de St. Étienne de Bordeaux date de la période 650-75, période assez tardive, sans doute aussi vers 650. Un exemplaire de cette pièce faisait partie de la trouvaille de Bordeaux étudiée par Pierre Le Gentilhomme ; la date d'enfouissement de ce trésor était très bien daté de 675-80. Peut-être votre trouvaille est-elle un peu antérieure (660-70), mais il n'est *pas possible* de la dater de 625. L'absence de deniers d'argent et de sceattas anglo-saxons la place bien antérieurement à 700 '.

It may be remarked here that the Sutton Hoo hoard in fact contains no coins of 'pale gold', in the sense of obviously base metal. Some of the coins are paler than others, but these include some of the pseudo-imperials, i.e. the earliest coins in the hoard. No evidence of chronological value can be derived from colour-distinctions within the hoard.⁵

M. Lafaurie thus says that it is 'hardly possible to put the date of the burial before 650' and suggests that some of the coins indicate a later date. Perhaps the date of burial, he says, may be a little before that of the Bordeaux hoard (675-80), that is to say it may be 660-70, but 'it is *not possible* to date it to 625'.

Now what are these numismatic opinions worth? There is a natural enough tendency amongst laymen to swallow numismatic verdicts whole, and to place more weight than they deserve upon coins as a dating factor in fields of numismatic study which are in fact not yet, and may never be, susceptible to precise datings. Sceptics in 1939 and 1940, in the persons of Mr Crawford and Mr Dayrell Reed, did a service in pointing out—and rubbing in—the uncertain nature of the numismatic evidence in this instance as an instrument of firm and precise dating. But there is now a tendency to go too far in the opposite direction and wave the evidence of the coins aside. The broad picture derived from the 37 Sutton Hoo coins, without laying undue stress on any one, is really not to be set aside. In his observations in an accompanying article, Mr Grierson replies to Dr Ward's strictures on Merovingian numismatics in general and to some of his particular points with reference to Sutton Hoo coins, and re-establishes the broad value of the dating criteria available. I must confess, however, that I personally as a layman do not feel convinced by Dr Allan's arguments for the exceptionally late date of 680. Without stopping to consider the historical and archaeological facts which render it highly unlikely, I cannot see the logic of some of his numismatic points, and must raise one or two issues with regard to his commentary.

³ i.e. beyond those previously identified by Dr Allan as belonging to specific mints.

⁴ Its appearance in Suffolk and burial there would of course be later. (R.B.M.)

⁵ Dr Plenderleith and Dr Moss, of the British Museum Research Laboratory, have also pointed out to me that the phenomenon of 'surface enrichment' in antique gold, whereby the surfaces of gold-alloy will turn to pure or nearly pure gold, by the working out from the surfaces, over long periods of time, of other constituents of the alloy, renders deductions from surface appearance or analysis of surface scrapings, extremely precarious. Specific gravity tests are much more reliable as indications of the baseness or otherwise of antique gold coinage.

Dr Allan does not regard the Sutton Hoo purseful as an ordinary hoard buried in a time of unrest and 'reflecting the currency at the moment of burial' but suggests that the coins came together in a continental moneyer's or tax collector's office on the continent, and that it was looted from there. The distinction is important. A looted hoard might be kept as a memento intact and unmixed with any local currency for some time; and might later be buried, still intact, either still as a memento of the episode that led to its looting, or simply as treasure. Dr Allan's date of 675 for the composition of the hoard would represent the date of the raid on the continental moneyer's office, not the date of the ultimate burial of the coins in East Anglia, which could be presumed to be some little while later. Again, if the hoard remained intact as a piece of loot, not reflecting the currency at the time of its burial, the absence from it of Anglo-Saxon sceattas or gold coinage need not imply that the burial must pre-date the emergence of the sceatta coinage or lie outside the area of circulation of Anglo-Saxon gold. It need only imply these conditions for the locality and date of the looting, which tells us nothing.

Dr Allan's reason for supposing the Sutton Hoo hoard to have been looted from a continental moneyer's office is that along with continental coins (which alone are present) were the two ingots and what he presumes to be three defaced coins, i.e. coins once struck but hammered blank, perhaps for re-striking. These billets and flans he regards as objects that would only occur in a moneyer's or a tax collector's office.

This does not seem to me to be necessarily, or even probably, correct. It should not be assumed that moneyers or tax collectors were the only people who worked with small gold ingots or hammered out unwanted coins into plain, even discs. Goldsmiths no doubt used such small billets in fashioning the gold minutiae of the period—pins, barrel-beads, thin strips for folding into the cell-walls of cloisonné work, wire and granulation for filigree work, and so on. The Sutton Hoo coins could have been accumulated in Suffolk as bullion or treasure, the blanks and ingots being simply additional bullion, gold in transition from coinage into jewellery. It is in any case not certain that the blanks are hammered-out coins at all; Mr Grierson has pointed out to me that there were similar blanks in the Crondall hoard, and that they may well be evidence of a habit of locally supplementing a meagre supply of stamped coins by unstamped pieces of metal of the appropriate weight. If the blanks and ingots can be explained in this way, it is no longer necessary to consider the hoard as loot from a continental source. The coins, though foreign, could have been in circulation in East Anglia. Merovingian gold coins were circulating freely in Kent and South England, and it would be very surprising if they were not also in circulation in Suffolk, the connections of which, especially the South-east, with Kent and the continent were at this time close, as the heterogeneous contents of the Sutton Hoo burial itself shows.⁶

There is also something to be said on strictly numismatic grounds against the idea that the hoard was assembled on the continent. Mr Grierson has put the point in correspondence with the writer as follows:—'If it had been collected together on the continent at all, it would show some kind of local concentrations, like the groupings in the Bordeaux hoard, the presence of a number of identical coins of the same local mints in the Mons hoard, and the Quentovic and English groupings in the Crondall hoard. One would expect these groupings even if the hoard had been brought back by an English merchant: there would be some concentration to show where he had traded or spent his time'. Again, the coins were buried in a sumptuous purse of local East Anglian

⁶ See R. L. S. Bruce-Mitford, 'Saxon Rendlesham', *Proceedings of the Suffolk Institute of Archaeology*.

manufacture. True, this may have been used to carry rings, jewels, ingots and other treasure, such as Germanic kings dispensed, but the presence of the coins in a native purse seems rather against the idea that they are loot and in favour of the idea that coins (Merovingian ones, as in Kent) were in circulation. No rings or jewels or other treasure were in fact in the purse, beyond the ingots, which may have been added as a bullion reserve at the time of the burial. The Crondall purseful of coins (101 in number, including one forgery and three blanks, and with no other objects like jewels or rings) tends to reinforce this feeling. Finally, there is no historical evidence, or background, for Anglo-Saxon raids on Merovingian territory at this time.

If Dr Allan's view of the Sutton Hoo hoard as loot is not accepted, we may take the date of burial to be near enough the date of composition of the hoard; and we need not allow for any special time-lag; i.e. we can forget about 680+, and think rather in terms of 675+.

The view taken of the nature of the hoard has some important numismatic implications, as indicated above; but it makes only a little difference to the upper limit of date for the burial. On this point, other arguments of Dr Allan do not seem to me convincing. For example, it does not follow that the coins of Bordeaux, Châtre, Laon and Chambly, which have anchor-cross reverses, 'must be put after the direct influence of Eligius had gone, i.e. later than 660', merely because these reverse designs represent an evolution from a reverse design introduced by him perhaps as early as 628-30. Eligius died in 659, but we cannot assume that his influence remained dominant until the moment of his death. The mints referred to are not far, geographically, from Paris, where Eligius exercised his functions as mint master, but there is no reason to suppose that he exercised such influence over independent provincial mints that they could not develop or vary a design copied from one of his royal issues until he was dead. He did not control the provincial mints. Similarly, even if the busts on the coins of Chambly and Montignac are modelled on the royal bust that appeared on the coins of Clovis II, it does not at all follow that they must date 'long after Clovis II' (639-57). An early coin of Clovis might have reached a remote provincial mint, such as Montignac in the Bordeaux region, within a year or two of its issue, and degenerate or unskilful local copies of it could have been quite easily produced there actually some time before Clovis' death in 657. Indeed, one approaches Dr Allan's late dating of individual coins with the caution which judgments based on style demand, when coins were being struck in a very large number of small towns and villages and when moneyers varied greatly in competence and creative ability, so that good and very degenerate issues could be struck at different mints contemporaneously. It was always possible for a new moneyer to reform the dies of a mint, so that coins in good style might actually post-date and supersede degenerate ones. Such considerations may well explain the presence, in the Crondall hoard, of a few coins which at first sight appear to be later in date than any in Sutton Hoo.

At this point we may bring the third numismatic opinion into the picture. Since Mr Grierson's comments will be found in an accompanying article, it is only necessary to summarise them here. His conclusion is that on purely numismatic grounds the burial cannot be dated before 650, and that there seem to be good reasons for ascribing it to the decade 650-60. This, as we have seen, is a little earlier than the date (660-70) suggested by M. Lafaurie, though the latter does not definitely exclude a slightly earlier date. Mr Grierson observes also that it does not differ materially from that proposed by M. Le Gentilhomme. The latter favoured the decade 640-50, but on the assumption that the hoard had taken shape in Gaul; if, as seems likely, it came

together in England, it would be reasonable to allow for a short time-lag. All three solutions would regard 675+ as decidedly too late.

* * * *

From the foregoing accounts, though the coins themselves are not yet published some firm conclusions emerge. First, it is quite impossible to identify the grave with Redwald. Secondly, it no longer seems possible to consider any date for the Sutton Hoo burial before 650. Thirdly, numismatic opinion is not agreed in support of an earliest date as late as *c.* 675. The consensus of numismatic opinion points clearly to somewhere between 650-70. There is no numismatic evidence which certainly precludes a date in the decade 650-60, and Mr Grierson actually favours this date, rather than a later one, on numismatic grounds. The crux of the matter lies in Dr Allan's sentence 'More attention has been paid by students to the identification of mints than to the chronological arrangement of the coinage'. The two great standard works on Merovingian numismatics, by Prou and Belfort, appeared at about the same time, the first in 1894, the second between 1892 and 1895. Very considerable original research on the dating of the Merovingian series is needed, with the 37 Sutton Hoo types as the centre of study, before what can be described as a final dating of the burial can be arrived at. This task I am glad to say is being undertaken by Mr Grierson. Meanwhile, a date of burial after *c.* 650 must be accepted, and when to the evidence of the coins, archaeological considerations and the known facts of seventh century East Anglian history are added, as described in the paper in Suffolk Institute Proceedings referred to, it seems to me that this date cannot be long after 650. If so, the identification of the Sutton Hoo monument with one of the three brothers Aethelhere, Anna and Aethelwald seems in the highest degree probable.

With regard to Dr Ward's other arguments, there is no point in mentioning everything with which I disagree. He does not, however, seem to appreciate the difference between evidence and hypothesis. There is no evidence that the spoons belonged to Redwald, though it is quite likely. But even if they did, and the standard also, it in no way follows that the grave they were buried in is his grave. In my article I say that they may have survived in the royal treasury, or been handed on to his son or a nephew. Again, Dr Ward ignores (*op. cit.*, p. 11, item 3) the evidence that there was no body in the burial. Does this fit with Redwald? His items 3 and 4 simply indicate that he has not taken in my treatment of these subjects. His item 5 I cannot understand at all.

The truth is that there is a quite simple explanation of the size, richness and distinction of the grave. It is the first intact royal inhumation ever found. Of course it is bigger and better than previous graves. But this in no way proves it to be Redwald's. My comparison with the royal cremations at Old Uppsala and with Childeric's grave suggests that its furniture was nothing very out of the ordinary as a royal grave of the era. Nor can I agree with Dr Ward that were it not for the alleged evidence of the coins the grave would be unhesitatingly identified with Redwald. Purely archaeological considerations and particularly the gold jewellery with its affinities with the Book of Durrow, seem to me to suggest a later date than 625-6. These points, however, need not be pressed, since the coin evidence in fact precludes Redwald.

The Dating of the Sutton Hoo Coins

by PHILIP GRIERSON

DR GORDON WARD'S summary of the evidence of the coins and of the views of numismatists regarding the date of Sutton Hoo calls for some comment. Its object is apparently that of demonstrating that, as he himself puts it, 'the numismatists have been set a task for which they are not equipped'. The archaeologists, he suggests, must be left to settle the date between themselves, and the numismatic 'evidence' can simply be ignored. If this 'evidence' runs counter to his belief that the burial commemorates King Redwald, so much the worse for the 'evidence'. It is too uncertain and imprecise to be of much use anyway.

Dr Ward is not entirely to blame for having succumbed to this opinion. It is largely a consequence of some rather hasty remarks on Merovingian coinage in general, and the Sutton Hoo coins in particular, published by Mr Derek Allen in 1939, shortly after the acquisition of the coins by the British Museum. These remarks were subsequently commented on by Mr Dayrell Reed in terms which suggested that the evidence of the coins was so vague as to be valueless. Although the negative views of these two scholars were countered by M. Pierre le Gentilhomme, who had embarked in the thirties on the study of the chronology of Merovingian coinage and had made more progress in this field than all his predecessors combined, he was not able to develop his views at length before his death, and the exact import of those he had expressed was missed by subsequent commentators. The result was that when in 1940 the Editor of *ANTIQUITY* summed up the various opinions that had up to then been put forward, he felt justified in indulging in the cynical comment that 'where such a range of possibilities exists, a difference of five or ten years cannot be detected, and that is all that separates the numismatists from their archaeological and historical colleagues'.

Mr Derek Allen's distinction as a numismatist is so universally admitted that no harm can be done to his reputation by saying that his article on the Sutton Hoo coins is not his best work. He was suddenly called upon to deal with a number of coins in a series of exceptional difficulty, with which he had no previous familiarity, at a time when men's minds were concentrated on far from academic matters and when contact with foreign scholars and access to foreign collections was no longer possible. The consequence was that he committed himself to three statements of a very misleading character. The first was that in regard to the dating of Merovingian coins, 'unless the king's name is present we have nothing but style to guide us'. The second was that a coin in the Sutton Hoo hoard which he ascribed to Theodebert I (534-48) was a 'later copy of one of his, and may not have been struck till many years after his death'. The third was that 'from the evidence of the coins, the Sutton Hoo burial cannot have taken place before about A.D. 600, is not likely to have taken place earlier than A.D. 640-50, and might even have taken place nearer A.D. 670'.

The second of these points can be disposed of at once, since it is irrelevant to the main issue. It is important only because the suggestion that the coin is a forgery has been subtly used to create a prejudice against the evidence as a whole. In fact, the coin is not one of Theodebert I, but of Theodebert II (595-612). The obverse legend is identical with that of coins of Theodebert I, but this is of no more significance than is, say, the identity of legend on coins of Edward III and Edward I; it is a consequence of

the fact that both sovereigns had the same name. The lettering of the coin is quite characteristically that of coins of the moneyer Manileobo, who minted for Theodebert II at Clermont-Ferrand, and who clearly revived an old legend for his master. Although the coin has frequently figured in subsequent discussions of the date of the hoard, it has no relevance at all in this connection.

Much more important is Mr Allen's statement that style is the *only* criterion available for dating non-royal Merovingian coins, for the usefulness of style in this connection was strongly criticized by Mr Reed. He contended, in the first place, that it was much too vague. There might be an error of as much as thirty years in either direction, and if the coins corresponded fairly closely—Mr Allen had noted that they did—with those of the time of Clovis II (639–57) this would mean no more than that 'they might have been struck at any time between 615 and 675'. In any case, Mr Reed argued, the large number of mints involved must to a considerable extent invalidate stylistic arguments altogether. The competence of the workmen employed in different mints would be too disparate for one to be able usefully to compare their products.

The second of these considerations has considerable truth behind it, though with the necessary proviso that however early a date we may suggest for an imitation, no amount of reasoning can make an imitation earlier than its archetype. In view of the generous latitude in chronology allowed by Mr Allen and Mr Reed, this proviso is a very relevant one. The hoard *cannot* be as early as 600 (Mr Allen), or even 615 (Mr Reed), for the archetype of the 'anchor-cross' series, of which there are seven derivatives in the hoard, did not antedate the 620's, and the 'chalice' series of Banassac, two degenerate specimens of which occur in the hoard, only appeared, so far as our knowledge goes, under Charibert II (629–32).

The imprecision of stylistic evidence is also greatly exaggerated by Mr Reed. It must be remembered that in comparing one hoard with another of a different date, the *average* composition is bound to be different, and stylistic criteria are here both relevant and reliable. One would expect a hoard dating from between 615 and 639—for such dates Mr Reed would be prepared to allow as possibilities—to contain a fair proportion of the 'Victory' types of the sixth century and a considerable number of 'Maurice Tiberius' coins, to say nothing of stray Byzantine pieces. By the middle of the seventh century these would have almost, if not quite, disappeared. If the average composition of the Sutton Hoo hoard corresponds (as it does) to the general run of coinage current in the time of Clovis II, we need have no hesitation in ascribing it to the period of this monarch's reign.

The real fallacy of Mr Reed's argument, however, lies in the fact that he has taken too literally Mr Allen's statement that the chronological classification of the non-royal Merovingian coins can only be determined by considerations of style. There are other methods to be employed as well. There is the evidence of a few hoards which, owing to the presence of Byzantine or Visigothic coins in them, can be dated with precision, and which, like the names of sovereigns on the 'royal' series, give the numismatists certain fixed points from which to work. There is also the evidence of die relationships, and that provided by the work of individual die-sinkers who were employed in more than one mint.

For example, Sutton Hoo contained a coin of Usson struck by the moneyer Disiderius. There are two other coins of the same moneyer in the Cabinet des Médailles in Paris. All three have different reverse types. A comparison of the Sutton Hoo coin with one of the Paris specimens shows that they have a common obverse die, which was much fresher when it was used for the Sutton Hoo coin than it was when used

for the Paris coin. The Sutton Hoo reverse must therefore precede the Paris one in date. The obverse of the third Paris coin is copied, not very skilfully, from the obverse we have just been considering; the reverse of this coin must therefore be later than the two other reverses. Here, quite simply, a study of the dies has allowed us to place three reverse types in their correct chronological order, and it is not irrelevant to note that it was the third and latest of these coins that was included in the Bordeaux Hoard of c. 680.

Again, the coin of Huy in the hoard links up through its moneyer Landigisilus with a die-sinker who made dies for Betto at Soissons, Theudegisilus at Metz, Arnulf at Strasbourg, and other moneyers elsewhere. For some of these mints we have not sufficient material to allow us to establish a clear chronology. But for the more prolific mints this is a quite feasible task, and the interlocking of types and the activities of individual die-sinkers can be used in many cases to bring the minor mints into the picture. Even where relatively unimportant series are concerned, we can in this fashion arrive at something much more definite than the 'not earlier than 600, not likely before 640-50, and possibly as late as 670' of Mr Allen, and the 'any time between 615 and 675' of Mr Reed.

The evidence on this point is as a whole quite clear, even though precise dates cannot be given for individual coins. The majority of the coins in the hoard belong to the second quarter of the 7th century. The anchor-cross type already alluded to, for example, was inaugurated at Paris during the reign of Chlotar II (613-29), and started as a cross with an omega above and an alpha below. This was quickly simplified by the omission of the alpha and the fusion of the omega in varying degrees with the cross. In the 630's and 640's it became one of the commonest types over a considerable area of west-central France. There are seven specimens in the hoard, and they can certainly be ascribed to the second quarter of the century. One of the coins of this type has the bust described as 'à l'appendice perlé', the origins of which are dated by French numismatists as c. 640-5.

M. Le Gentilhomme stated all this quite clearly in 1940, and only special pleading of the most determined kind has enabled later scholars to discount what he said. 'L'activité de ces derniers ateliers'—he is referring to such rural mints as La Trémouille, Usson and Montignac in Poitou and the Limousin—'et la présence de la croix dite ancrée sur les monnaies de Saint-Etienne de Bordeaux, de Laon, de la Trémouille, du fisc et sur quelques pièces indéterminées, [nous] obligent à descendre à l'époque de Clovis II, caractérisée justement par l'éparpillement des ateliers ruraux et la faveur de ce type de croix'. The reign of Clovis II means 639-57; it does not mean a decade or two earlier.

This, however, is only the earliest possible date at which the burial could have taken place. With regard to the most likely date, some hesitation is possible. M. Le Gentilhomme favoured the first half of the reign of Clovis II—i.e. the 640's—on the ground that the 'anchor-cross' type, though important as an element in the hoard, was not yet the predominant one on coins coming from Neustria and Aquitaine. To this view there are two objections. One is that while the argument would be valid if the hoard had been put together in France, it loses its cogency if the hoard had come together in England, as there are good reasons for supposing it to have done; a time-lag between the striking of the coins and their crossing the Channel would explain why the 'anchor-cross' types do not yet predominate. A more important consideration is that while there is no coin in the hoard that can be ascribed with certainty to a date posterior to 650, there are several that must have been struck about that time.

On the other hand, a date of burial appreciably later than 650 is rendered unlikely by the presence of as many as six coins—a partial imitation of Justinian (527-65), three

imitations of Maurice Tiberius (582-602), a coin of Theodebert II (595-612) and another of his moneyer Manileobo—which can be ascribed with certainty to the end of the 6th or the opening years of the 7th century. These coins are all different from one another, and are certainly not a secondary hoard of the type which, when incorporated bodily in another hoard at a later time, can lead the student far astray in matters of dating.

A provisional conclusion is that the hoard cannot be earlier than 650, since it contains a number of coins which any numismatist must ascribe to the 640's. On the other hand, it is unlikely to have been buried at any date later than 660, both for the reason just given and because it contains no obviously late types or any specimens of the debased gold which became normal towards the end of the century.

Important New Books and Articles

The inclusion of a book in this list does not preclude its subsequent review

PREHISTORIC EUROPE: the Economic Basis, by J. G. D. CLARK, with 16 plates and 180 text illustrations. Methuen, 1952, £3. [A major contribution to the subject which will be reviewed in due course].

MEMORIAL VOLUME TO SIR ALFRED CLAPHAM: *Arch. Journ.* vol. CVI, supplement. R. Arch. Inst., 37 Onslow Gardens, s.w. 7, 12s 6d to non-members [Essays by his friends chiefly on architecture and all of a high order of scholarship].

BEGINNING IN ARCHAEOLOGY, by KATHLEEN M. KENYON. Phoenix House, London; 12s 6d. [An authoritative book for beginners].

BOATS AND BOATMEN, by T. C. LETHBRIDGE: Thames and Hudson. 15s. [No arm-chair stuff and, like all he writes, most eminently readable: very strongly recommended].

THE HERITAGE OF EARLY BRITAIN, by various authors. G. Bell, 12s. [Yet another excellent book of a popular kind].

DEVONSHIRE STUDIES, by W. G. HOSKINS and H. P. R. FINBERG. J. Cape, £1 16s.

CASTLE DORE. Report on the excavations at, by C. A. RALEGH RADFORD. *Journ. R. Inst., Cornwall*, n.s. vol. 1, 1951.

DISCOVERING THE PAST, by JACQUETTA HAWKES. Nat. Council of Social Service, 26 Bedford Square, London, w.c.1, 2s. [A popular handbook explaining the techniques and raw materials of archaeology for beginners, so far as this is possible in 15 pages. It should have been longer].

ARCHAEOLOGICAL SURVEY IN THE LOWER MISSISSIPPI VALLEY, by P. PHILLIPS, J. A. FORD and J. GRIFFIN. *Peabody Museum Papers*, vol. XXV, 1951.

Notes and News

CELTIC CHARIOTS ON ROMAN COINS

The purpose of this note is to draw attention to representations of Celtic chariots on Roman coins which have not hitherto been adequately discussed or illustrated. Sir Cyril Fox, in his Anniversary Address to the Society of Antiquaries in 1947, reproduced an enlargement of Déchelette's illustration of a coin of the Remi which represents a chariot of Celtic type with semi-circular side-screens¹, but there are other representations which, since Beale Post drew attention to them in 1853², have not been discussed from the point of view of the archaeology of the vehicles represented upon them.

The coins are illustrated by enlarged photographs in PLATE I. The first (top left) is a denarius signed by Scaurus³ and usually attributed to 92 B.C., but Professor Michael Grant points out to me that a case has been made out for ascribing it to 118 B.C. and the foundation of the *colonia* Narbo: a date to which he himself inclines⁴. At all events the attribution of the warrior on the chariot as Bituitus, ruler of the Arverni, seems likely enough. He is shown naked, and holding spear, long shield and *carnyx*. The two-horse chariot has no side-screens but appears to have an open platform with perhaps some sort of wrapping obscuring the feet of the warrior.

The second coin (top right) is of the moneyer L. Hostilius Saserna⁵, of the last 50's or first 40's B.C.—a date of c. 48 B.C. has been suggested. Here the chariot has the semi-circular side-screens of the Remic type, with a naked charioteer crouching well forward, the reins in his right hand and a whip in his left, while behind him stands a naked warrior with dishevelled hair facing to the rear and carrying spear and long shield. This figure and the head on the obverse have sometimes been attributed to Vercingetorix.

The third coin is one of Julius Caesar⁶, and Professor Grant thinks c. 49 B.C. a likely date; although it has been assigned to a Spanish mint, he does not feel that such an attribution is altogether secure. A trophy is represented, arranged on a tree-trunk, by which is propped a chariot, pole-tip against the trunk, with a pair of side-screens of the same semi-circular type as those already noted.

Now while these coins provide welcome iconographic data on chariots which must be Celtic, they (and the Remic coin published by Déchelette) raise chronological problems of some interest. Literary evidence is consistent in showing that the Celtic tribes waged chariot warfare in Italy from the Battle of Sentinum to that of Clastidium (295-222 B.C.) and again in the Delphi raid of 279 B.C., while in Transalpine Gaul Bituitus is specifically described as having a silver-fitted chariot—*argenteo carpento*

¹ *Antiq. Journ.*, xxvii (1947), 117-19, with Pl. xviiiA, from Déchelette, *Manuel* II, 1190, fig. 504.

² *Journ. Brit. Arch. Assoc.*, vi (1850), 252-5.

³ *B.M. Cat. Coins Roman Repub.*, I, p. 184, no. 1185.

⁴ *Num. Chron.*, 1924, 31 ff.; 1941, 120 ff. I am most grateful to my colleagues, Professor Michael Grant and Dr John Allan, for their help in this matter. Dr Allan kindly provided the casts from which my illustrations were made, while still in the British Museum.

⁵ *BMC Repub.*, I, p. 513, no. 3994.

⁶ *Ibid*, II, p. 363, no. 70.

qualis pugnauerat—in 121 B.C.⁷ We may therefore accept the denarius of Scaurus as likely to be a contemporary representation of the Gaulish warrior and chariot. But by 58 B.C. the total absence of any reference to chariot warfare by Caesar, and his surprised recognition of its survival in Britain a few years later, shows that by that date it had been dropped from tactical use by the Gauls.

It is therefore difficult to accept the Hostilius Saserna coin and that of Julius Caesar, both within a year or so of 50 B.C., as representations of contemporary Gaulish chariotry or the reverse of the former as depicting Vercingetorix, and this also applies to the coin of the Remi, again of mid-first century date. Dr Allan, commenting on this coin⁸, points out that whereas a chariot could have been copied from a Roman denarius of any date from 150 B.C. onwards, the Remic representation is specifically that of a definite chariot type, known to be Celtic and non-Roman, and not merely a vague approximation to a two-wheeled vehicle with its horses.

We seem left with the possibilities that either the three representations of the chariot with semi-circular side-screens are deliberate archaisms, or, at least with the Caesar and Hostilius Saserna coins, that they represent British vehicles and are not unconnected with the campaigns of 55 and 54 B.C.⁹ It is at this point that the problem must be handed by the archaeologists to the numismatists.

STUART PIGGOTT.

A CIRENCESTER TILE

In 1923 an inscribed, clay-baked, roofing tile, found in Cirencester, was placed in the old Bathurst collection of local antiquities, and is now in the new Corinium Museum.

The inscription on the tile has been variously dealt with. For convenience these readings are numbered.

(1) Mr St. Clair Baddeley and Mr Collingwood deciphered it (*Journal of Roman Studies*, no. XI, p. 239) as CLLT XVIII, explaining this as,

Cl(audius ? and) L(ucius ?) (made 19 T(iles)).

The sense of this interpretation—two workers, so little work—left the field open for further inquiry.

(2) A suggestion of my own, published in a local paper at the time, was that the inscription might be read as CETUS 4, implying that a Romano-British tilemaker Whale made four batches of tiles; how many per batch is not known. The cursive 's' is scratched, as I conceived it, across the stem of the T. In this (possessive) connection, a nominative is found as well as the commoner genitive. Further, to take the ligated T as standing for tegulae (tiles) seems unlikely; one could see the articles were tiles.

I submitted this attempt to Mr H. B. Walters, Roman pottery expert, in the Greek and Roman Antiquities Department, British Museum, who replied, 'The inscription of which you enclose a reproduction appears to be in the ordinary Roman cursive

⁷ The literary evidence is conveniently set out by Arbois de Jubainville in *Rév. Celtique*, ix (1888), 387–93 and Theodore Reinach in *ibid*, x (1889), 122–33. De Jubainville gives the Bituitus reference, from Florus.

⁸ In Fox, *loc. cit.*, 118 n.

⁹ Professor Grant notes that it has been suggested by Babelon that the 'Vercingetorix' representation may in fact be a personification of Pallor or Pavor (*Monn. Rep.* i, 549–52), and that an alternative explanation of the reverse would be a reference to a battle fought by Hostilius' legendary ancestor, Tullus Hostilius, against the people of Veii (*ibid*, i, 549). The head may be nothing more than a typified Gaul. Babelon further referred to a British *essedum* when commenting upon the trophy on the coin of Julius Caesar (*ibid*, ii, 13).

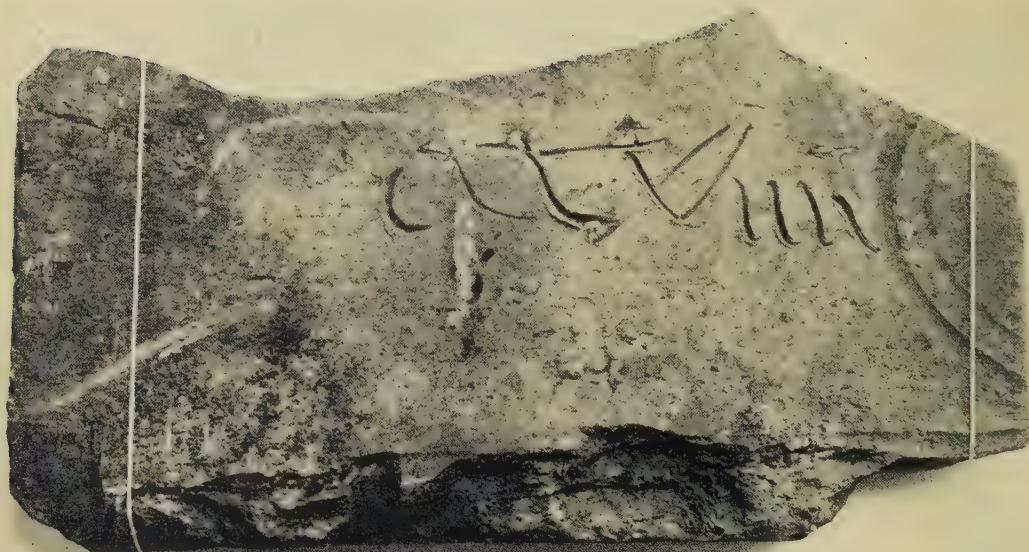
PLATE I



ROMAN COINS, LATE SECOND AND FIRST CENTURY B.C., SHOWING CELTIC CHARIOTS ($\times c. 3$)

Ph. P. Glasier

PLATE II



TEGULA DUROCORNIVIANA

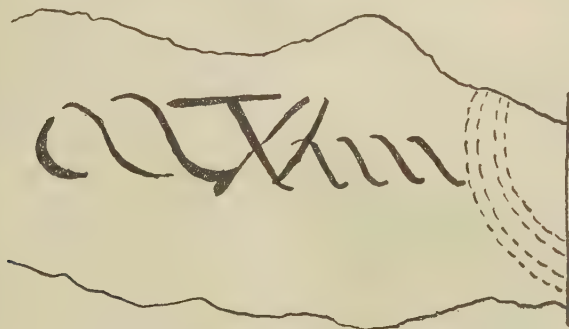
Ph. Moss, Cirencester

characters, and your reading CETU(s) is certainly quite possible'. With regard to the reading quoted above, he commented, 'I should be quite prepared to accept it as far as the TXVIII—it would account more satisfactorily for the ligated letters—but I cannot accept the CLL without considerable hesitation. The characters certainly seem to me to me to be CE'.

(3) Pursuing the matter I wrote to Dr van Hoesen of Princeton University, author of a manual on Roman cursive writing, which he kindly lent me. He explained that his duties had kept him out of touch with palaeography for several years, and he would only risk 'a hasty, off-hand suggestion'. This was that the inscription might be read CCCXXVIII tiles, 324.

With due deference, I do not think this can be supported: if the first stroke is 'c', the next two cannot be; and conversely.

(4) I next laid the problem before Prof. Cumont, the eminent Belgian authority. He would not venture an opinion, but forwarded my letter to an expert here, his friend Prof. Anderson, who replied regretting that neither M. Cumont nor himself could throw



any light on the point. He remarked cheerfully that when the tile inscriptions were deciphered not much was gained.

(5) A lady specialist whose opinion I next sought, replied, 'The more I look at the tile the more I am inclined to begin with the ominous letters BL'. Her solution for what it was worth, she suggested, would be 19 bloody tiles.

(6) On a visit to Corinium Museum in 1950, and making as usual for the old tile, I discovered it with the label attached:—'ARTUCI; in cursive script, a potter's name. Deciphered by Felix Oswald'. As we have to do with a tileyard scribble, and not a potter's stamp, this was patently wrong apart from obvious mis-reading of the two end letters.

Thoroughly to clear the matter up I wrote to Dr Felix Oswald who replied:—'I have no record of it, and I fear this is an error, and some one else must have deciphered it and erroneously labelled it as by me'.

(7) That disposed of, I was still unable to let the quest drop which has pleasantly haunted me for more than 20 years. An enquiry at the British Museum's Dept. of Brit. and Med. Antiqq. in 1950 resulted in the following:—

'We think it is a numeral CCCXVIII, or 319. It was probably scribbled on the last tile of a batch to show the total number in the batch'. Signed: J. W. Brailsford, Assist.-Keeper.

The same adverse criticism holds equally here as in No. 3. It is noticeable however that Nos. 7 and 3 correspond closely.

(8) Visiting the Museum again in Sept., 1951, I found yet another attempt on the graffito, viz., ALTUM. Its author, Mr R. P. Wright, M.A., F.S.A., remarks, 'the upper part of the tile has been broken away, so we do not know what thing was "high"'. (See *Journal of Roman Studies*, No. XLI, 1951).

A serious fault in this case is the overlooking of the definite cross-stroke through the T-stem, and the assumption of lost letters on the broken-off part of the tile seems unwarranted. Further, the last 4 and smaller strokes (if M) seem not to form part of one and the same word.

The Cirencester tile guards its secret despite the exceeding clearness of its lettering : even the order of the strokes as traced in the wet clay can be recovered from the complicated TXV part of the inscription. The trouble is we are not sufficiently versed in Roman cursive to pronounce definitely, otherwise Cumont would not have fought shy of the problem. But this much is certain, whether the scribe was foreman or workhand, whether he used style or twig—his name was not ARTUCUS.

J. B. JONES.

EXCAVATIONS AT BODRIFTY, MULFRA HILL, CORNWALL

Bodrifty settlement lies 4 miles north of Penzance, on the western slopes of Mulfra Hill. The site is at the head of the valley reaching from Porthmeor, on the north coast, to the high moors. The area is very rich in prehistoric remains and a ridgeway touches the site.

The settlement consists of many huts, lynched fields and enclosures. There is also a ring wall,¹ much mutilated, enclosing about 4 acres of the site, including the area being excavated ; much of this is in a furzy croft.

The largest hut circle examined (25 feet across) appears to be of the Iron Age, but the site was probably occupied by an earlier dwelling of the Late Bronze Age. Excavation has shewn that the site of the hut was cleared and levelled by cutting back into the hillside and spreading the excavated rab over the lower part of the floor. Finds occurred in this redeposited rab. Walling set into this make-up would be likely to collapse and this has in fact happened in certain places. The wall of local coarse granite is very wide (8 feet) ; it is much robbed, but appears to consist of a normal wall of about 4 feet, faced on both sides and backed by another wall, also of about 4 feet, but faced on the outer side only.² Most of the stones are laid on their long sides, an Iron Age feature locally ; there is no mortar and no real coursing, except on the north side of the five-foot wide entrance. This coursing is unlike the walling found in the other huts examined or that of the huts at Trewey-Foage, a Late Bronze Age site near Zennor.³ The outer wall appears to be a later addition for a small hearth with a number of sherds was found in the space between. It was probably built to keep out the wet. The hut is very damp and a deep water channel runs along the upper side, where the wall is thickest ; between the kerbs of the channel and the wall face is a ' duck walk ' of flat paving slabs.⁴

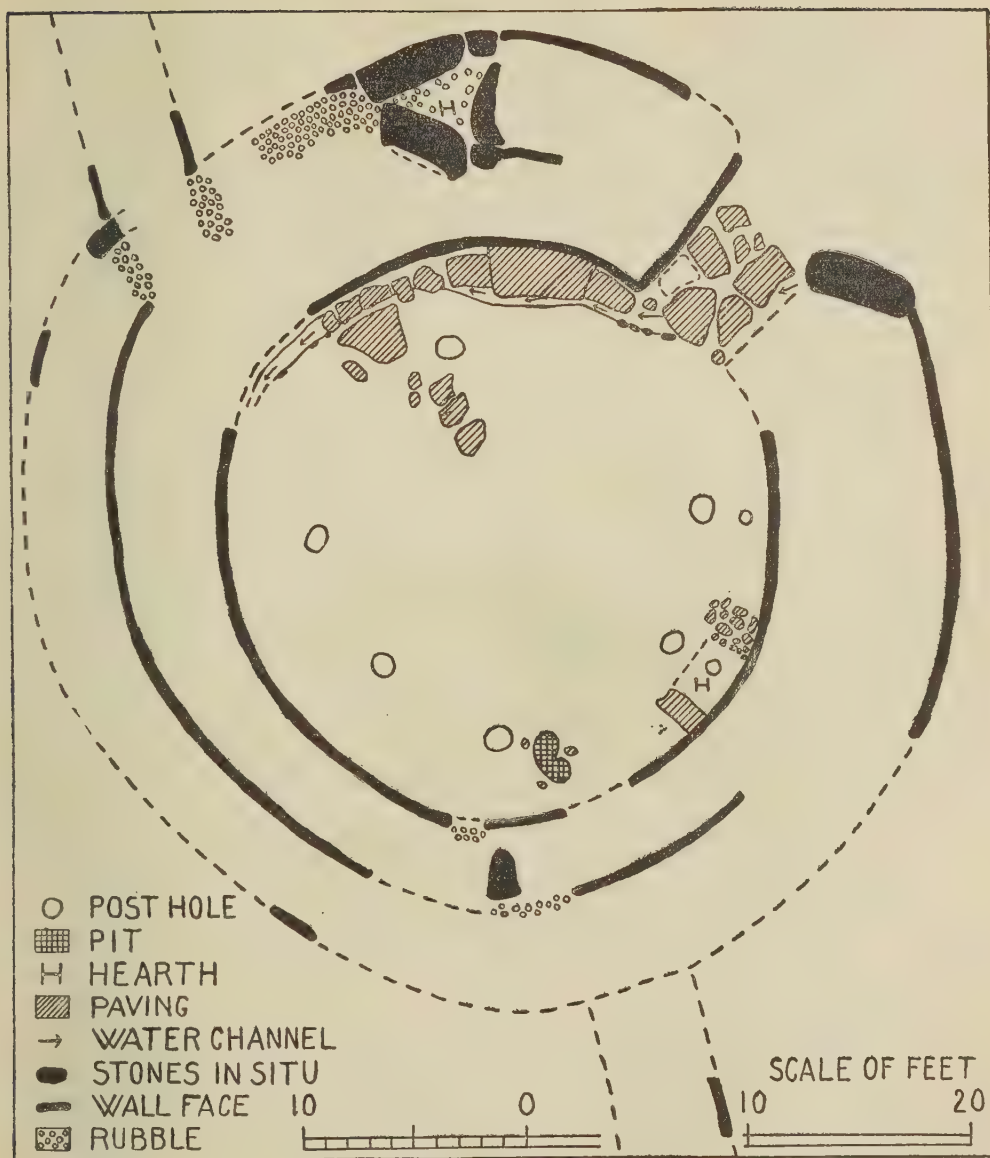
The roof of the hut was supported by a ring of posts, set in holes cut into the floor. These are six in number and average 13 inches in diameter and 12 in depth ; they are set

¹ It is hoped to investigate this next season.

² Much spoiled by stone robbers.

³ *Arch. Journ.*, xcvi, 105.

⁴ The excavation of the floor is not finished.



IRON AGE (A) HUT CIRCLE AT BODRIFTY, CORNWALL

3 feet in from the wall. The posts were in several cases packed with stone and four of the holes produced sherds of the Bronze or Iron Age. There is no evidence of a central post or of a fireplace. Two good stake holes suggest that one part of the outer ring was cut off by partitions running between the posts and the wall. One of these stake holes is overlaid by a later hearth with some paving alongside. This was associated with traces of iron working and is of later date.

The finds include a saddle quern, two mullers, various rubbers and whetstones, flints, beach pebbles and several spindle whorls of a very gritty pottery. These were found at a low level and resemble some from Plumpton Plain.⁵ Over 300 sherds were found at various levels, including fragments of the Late Bronze Age (Deverel-Rimbury type) and Iron Age A pottery like that from Dainton, Devon.⁶

Huts of this type, with an inner ring of posts supporting the roof have been found on a number of Iron Age sites including Maiden Castle.⁷ In Cornwall they are characteristic of the latest pre-Roman settlement at Castle Dore.⁸ The form of the roof is well illustrated by the reconstructed drawing reproduced by Dr Peate.⁹ This more primitive Finnish example shews a conical roof of poles supported on horizontal timbers carried by the upright posts. The lower ends of the poles there rest on the ground. At Bodrifty they would have rested on the inner edge of the hut wall as in the black houses of the Hebrides.¹⁰

D. DUDLEY

West Cornwall Field Club.

THE ORIGIN OF MAIZE

In ANTIQUITY No. 99, p. 157, I made some comments on Mr Carter's article on this subject. In it he mentions a paper by Stonor and Anderson which he regards as striking at the root of the problem. I now wish to call attention to a reply to this paper, which I believe to be a complete refutation of it, by P. C. Mangelsdorf and D. L. Oliver, (*Botanical Museum Leaflets*, Harvard University, vol. 14, no. 10, 1951, pp. 263-91) entitled 'Whence came maize to Asia?' Mangelsdorf is well-known as one of the foremost authorities on the origin and genetics of maize, and Oliver is Associate Professor of Anthropology at Harvard.

On the botanical side, the main points on which they differ from Stonor and Anderson are as follows:—

- (1) The maize found in the Assamese Hills is not unique among living varieties and comparable only with American fossil varieties. It is still found in America and elsewhere.
- (2) There is no record of maize in Asia before 1492.
- (3) The distribution of the varieties of maize in Asia does not imply that it was there before 1492. It can be explained better on the hypothesis of a more recent introduction.
- (4) The resemblances between sorghum and maize from Assam and from Bat Cave are superficial and of no significance in the argument.

On the ethnological side they show that the evidence quoted by Stonor and Anderson to prove the antiquity of maize in Assam has been misinterpreted and does nothing of the kind.

G.H.S.B.

⁵ *Proc. Prehistoric Soc.*, 1935, 32-3.

⁶ The pottery is still under examination.

⁷ R. E. M. Wheeler, *Maiden Castle*, p. 95, fig. 18.

⁸ *Journ. Royal Inst. Cornwall*, N.S. I, Appendix.

⁹ I. C. Peate, *The Welsh House* (Y Cymmrodor, vol. XLVII), p. 49.

¹⁰ ANTIQUITY, XII, 264.

THE 'DARK AGE' IN NORTH WEST INDIA

The two outstanding problems of prehistoric N.W. India are those of finding convincing links between the Indus and the Middle East, and of finding any evidence at all for the period between the end of the Indus Civilization¹ and the beginnings of sound history in India about 500 B.C.² Faced with these problems, archaeologists have tended to exhibit an academic *horror vacui* far more devastating than its natural counterpart. The most unlikely materials have been cast in to plug the breach. The prehistorian, for example, appears content with a handful of Harappan seals from Irak as evidence of trade and contact between the Sargonid realm and the unknown rulers of the Indus. Here—as in other archaeological fields—a background of work on a historically attested period seems necessary to maintain a balanced judgment when handling purely 'archaeological' material. The visitor from the court of the Caliph Harun-al-Rashid would have felt himself as much at home in the ports of Sind as does the modern European in Karachi. The envoy of Naram Sin or Hammurabi, by contrast, would have found the Indus Valley a strange new world, in some ways more advanced than his own, but in general more backward.

The latest attempt to fill this double breach is that which claims to find provincial variants of the culture of Sialk Cemetery B in the newly defined 'Londo' ware and the long-known Moghal Ghundi cairns of Baluchistan and which assigns a date about the beginning of the first millennium to these cultures. The theory has been advanced by Colonel Gordon and Miss de Cardi, and has received the seal of canonical approval from Professor Piggott.³ As a stop-gap it is undeniably attractive; but in one case it certainly will not hold, and in the other the cracks are menacing.

The Moghal Ghundi cairns were discovered by Sir Aurel Stein, who excavated a number of them.⁴ Some appeared to be cenotaphs; others yielded cremated bones and some grave furniture. The pottery was of brittle fabric, light pink inside, white slipped externally, and unpainted; with one significant exception, to which we will return, it has no determinate parallels outside Baluchistan. The metal objects found included three-leafed arrow-heads, and spear- and knife-points in iron; 'catbells',⁵ a tripod jar, and rings of bronze; and an expandable silver bracelet. In default of other evidence, the bells and the tripod jar have been related to those of Sialk B, and parallels for the three-leafed arrow-head have been sought in Anatolia. But it seems rash, to say the least of it, to seek parallels so far afield when they can be found nearer at hand. Scarcely two hundred and fifty miles from Moghal Ghundi, the Scytho-Parthian and Kushan city

¹ Dated nowadays to ca. 1500 B.C., e.g., by Piggott, *Prehistoric India*, 144: but this date may be as much as 500 years too low. (See, however, *ANTIQUITY*, XXIII, 201-5. Ed.).

² The detailed exposition by Pusalker of the dynastic traditions contained in the Puranas (Majumdar and Pusalker, ed., *The Vedic Age*, Book IV) serves only to show how valueless these traditions are for the historian.

³ For a general account, Piggott, *Prehistoric India*, 240; for Moghal-Ghundi, Gordon, *Man in India*, XXVII (1947), 234; for Londo ware, de Cardi, *Iraq*, vol. XIII, part 2 (1951), 63-75.

⁴ Stein, *An Archaeological Tour in Waziristan and Northern Baluchistan*, *Memoirs Arch. Survey India*, No. 37, 43-9.

⁵ The 'cat-bells' were not found during excavations and are only considered 'likely' to have come from the cairns; Stein, *op. cit.*, 47.

of Sirkap, Taxila, has produced arrow-heads and tripod jars⁶ which resemble those from the cairns at least as closely as do the similar objects from Sialk.

And there is other evidence which must be conclusive for the later date. Among Stein's finds at Moghal Ghundi he particularizes a bronze ring with an engraving of a helmeted male figure carrying a bow and spear, accompanied by a dancing woman.⁷ About this ring there is a conspiracy of silence among our modern commentators ; they come forward with no parallels from Nihavend ceramics or Assyrian glyptic. But this same silence may be interpreted as a tacit admission that Stein—who knew about these things—was right in assigning the ring to the Kushan or Gupta periods. And a similar date, or one only slightly earlier, may be ascribed to a pot decorated in relief with bunches of grapes, palmettes and figures holding festoons.⁸ These two details together with the general similarity of the metal objects from Sirkap and Moghal Ghundi, provide convincing evidence for dating the cairns, in round terms, to the period 200 B.C.–A.D. 200.

The second of our stop-gaps, Londo ware, is unfortunately known only from surface collections made by Stein and the equally intrepid Miss de Cardi. It is doubly unfortunate that only a small selection of pot forms and motifs has so far been published.⁹ But from a study of the Londo ware in the possession of the Archaeological Survey of Pakistan, it can be said at once that there is no resemblance between the fabric of Londo ware, which is very heavily grog-backed, and that of Sialk B, which is described as being fine¹⁰ ; that the distinctive forms of Sialk B, especially the 'teapot' do not occur in Baluchistan ; and that the leading Londo motif, the pot-hook spiral, is not known at Sialk. Other comparisons between the Londo metopic designs and those of Nihavend are too generalized to bear much weight. There remains one positive link between Londo and Sialk : a stylistic comparison between the horses on sherds from Londo and the animals on the Sialk 'teapots'. Now the subjective judgment of style would carry more conviction here had the objective identification of these animals been made correctly in the first place. Miss de Cardi calls them horses ; but extensive zoological research coupled with a Gallup poll among men of peasant stock suggests that horses are not, in the course of nature, provided with horns.

It may be added that the distribution of Londo ware also argues against its derivation from Sialk. The danger here lies in regarding archaeological finds as so many 'type-fossils' without reference to the activities of the men who used them and spread them. Stein's reconnaissances in S.E. Iran and Sistan¹¹ have shown that the Sialk cemetery B people did not migrate slowly from South West Persia to the eastern Makran, for such a migration would unfailingly have left its mark. Of course a 'horsey' people such as they were could, under pressure, have migrated with the greatest speed,

⁶ Three-leafed arrow-heads at Sirkap ; *Archaeological Survey of India, Annual Report, 1914-15*, pl. x ; tripod jars from Sirkap, *ASIR*, 1926-7, pl. xxviii, 1, and other unpublished specimens in Taxila Museum. Three-leafed arrow-heads have also been found at the Arab port of Bhambor, Sind, where they probably date to the 9th century A.D.

⁷ Stein, *op. cit.*, 48. Unfortunately Stein does not illustrate the design of the ring ; and the Archaeological Survey of India appears to have no knowledge of it.

⁸ Stein, *op. cit.*, 47-8 and pl. x.

⁹ In *Iraq*, 1951, with references to painted sherds previously illustrated by Stein.

¹⁰ The Sialk comparisons have been made with the material illustrated by Schaeffer in *Stratigraphie Comparée . . . de l'Asie occidentale*.

¹¹ Stein, *Archaeological Reconnaissances in N.W. India and S.E. Iran, passim.* ; *Innermost Asia*, chaps. xxix-xxx.

leaving no trace of their passage. We might then have expected their culture to be carried undiluted from the old ground to the new ; but the relation between the Londo and Sialk ceramics is most emphatically not that between, for instance, British and Continental Beakers. The remaining possibility, that these horse-breeders and cattle-rearers took ship down the Persian Gulf and sailed to the Makran is too incredible to notice.

In our present blissless ignorance, one suggestion may be made about Londo ware, but with diffidence. Though grog-backing was used for mixing basins and oven-pots in the Quetta and Saadat cultures, it does not appear to have been generally used for prehistoric wares. Here, as Miss de Cardi correctly observes, Londo ware is quite unlike any known prehistoric ceramic from Baluchistan. But a grog-backing does seem to have been general in a later pottery industry found around Quetta, and dated by Professor Piggott to the Sassan period.¹² Though the percentage of grog is lower and the shape of the particles differs from those in Londo ware, this Sassanian pottery may provide a clue for its dating.

In conclusion, we may suggest that stop-gaps, when too convenient or too readily acceptable, operate as obstacles to the acquisition of knowledge, which can only come by further and intensive attacks on the problems of the Indian dark age. With this in mind, we plead that the case for the derivation of the Londo and Moghal Ghundi cultures from that of Cemetery B at Sialk be judged ' non-proven '.

A. ALCOCK.

¹² Piggott in *Ancient India*, No. 5. Since this article was published further reconnaissances around Quetta have confirmed the attribution of the Lahore Museum pot to the Quetta region, and have yielded further samples of this fabric.

Reviews

PREHISTORIC INDIA. By STUART PIGGOTT. *Pelican Books*, 1950, pp. 1-293, plates I-VIII, text figs. 1-32. Price, 2s 6d.

Prehistoric India to 1000 B.C., to use the full title, is the scholastic harvest won from opportunities presented during some four years' military service in India. The achievement of this important book is a measure not only of Professor Piggott's stature as a prehistorian, but is a tribute to the excellence and applicability of British archaeological method as taught and practised at home. In the preface, the author rightly stresses that the book '... does not pretend to be more than a stocktaking of our incomplete evidence ...', but much original thought, practical work on the pottery, and field investigation give substance to the argument as it develops.

The main body of the work is concerned with the peasant and urban cultures of prehistoric north-western India and her western borders, the fate of these cultures and the advent of the Aryans. The first two chapters, however, provide background studies of the development of Indian prehistory, and of the earliest human inhabitation of the sub-continent. Readers who are not specialists in the Old Stone Age will be grateful for a very helpful summary of the palaeolithic in India, and the possibilities connected with the appearance of microlithic industries in what, in India also, may be called 'post-glacial times'. The third chapter is a splendid essay on the beginnings of food-producing economy, and the cultures expressive of this change, in Western Asia. A description of the sequences and inter-relationships of neolithic and early metal-using cultures of Mesopotamia and Iran follows, and this would deserve careful study for its own sake even if it were not included as an essential prelude to the comprehension of the Indian material.

In dealing with the pottery from ancient village sites in Baluchistan and North-western India, Professor Piggott has found it possible to follow D. MacCowan's classification of the Iranian painted ceramics, and he has distinguished a Buff-ware province, probably including three ceramic groups, or cultures, located mainly in south Baluchistan and Sind, and a Red-ware province in north Baluchistan. There is of course considerable interpenetration of these provinces. The more westerly connections of these cultures are discussed as is the evidence for trade between Early Dynastic Sumer and the Kulli culture of the Makran. The variety of burial methods amongst these painted pottery cultures is noteworthy, and fractional burial is represented.

The long and very full chapter on the Indus valley cities was fortunately not written too soon to miss taking account of Professor Mortimer Wheeler's report on his excavations at Harappā. The identification of citadels at Harappā and Mohenjo-daro, and the suspicion of town walls, now bring these cities into line with the general development of urban organisation in the Ancient East. The mature and static material culture observed throughout the successive phases of occupation in the two cities is peculiar, and indeed depressing, but it strongly suggests that they must have been intentionally founded as colonies of a parent city whose particular culture had already developed. In view of the apparent Red-ware affiliation of the Harappā pottery, the most likely location for this parent city is north-westwards, perhaps near Peshawar. A fascinating problem for future Indian archaeology is to explore the eastward influence of the Indus civilisation.

Surely the principal of zoned cultures, so well demonstrated around other city civilisations, took effect in northern India as well.

The final two chapters are of particular value for they bring together, and co-ordinate, information that has been even more inaccessible than that dealt with in the earlier chapters. Moreover their bearing is in many ways more direct on the interests of Old World prehistorians as a whole for they deal with the collapse of the great cities, the probable identity of the sackers, and the archaeological interpretation of the *Rigveda*. It would be tempting to elaborate a commentary on these matters, but it must suffice to say that the evidence embodied in Rana Gundai iv, Shahi-tump, the Jhukar culture, and Cemetery H at Harappā, points not to far-travelled Aryans but to barbarous peoples from the periphery of the Indus civilisation, who may have had a sprinkling of Aryan leaders (viz the shaft-hole axes), but who had become invaders on account of pressure in their own rear. The situation is similar to that so well known in Northwestern Europe where the ancient invaders of Britain were seldom the initiators of the movement but rather the outer waves of a disturbance far to the south. It seems to me that the close-knit socio-religious communities who composed the Aryans of the *Rigveda* have not yet been discovered archaeologically. The literary evidence for their exclusiveness, and their West Asiatic technological connections (chariot-wheels with spokes and metal tyres, cauldrons, etc.) to say nothing of their position in comparative philology, make it clear that they were a later and genuinely foreign intrusion to the Punjab. Material from Moghul-gundai and Jiwari, related to the horsemen's graves at Sialk B, does however point the way.

The book is admirably illustrated and the text figures include three maps. A special word of praise is due to the publishers for their promotion of knowledge in securing the writing of this book for their archaeological series.

T. G. E. POWELL.

'FORT' AT SCOTSTARVIT COVERT, FIFE, AND RECTANGULAR ENCLOSURE ON GREEN CRAIG, FIFE. By GERHARD BERSU. Reprinted from *Proceedings of the Society of Antiquaries of Scotland*, vol. LXXXII for 1947-8 (1950), pp. 241-75, fig. 13.

Since Dr Bersu's excavations in Wiltshire, at Little Woodbury before the War, gave a fresh start and great impetus to the study of prehistoric houses in the British Isles, he has himself carried out a notable series of excavations in the Isle of Man, Ireland, Wales and Scotland. Two of the most striking facts that he has demonstrated are the extensive use of timber for house building in the Iron Age, and the large size of many of the structures. Various workers since the war have shown that the first is true even in an area so well provided with stone as southern Scotland, but the most elaborate house plan there has been recovered by Dr Bersu's own skill, from tenuous indications in the sandy subsoil at Scotstarvit in Fife. He emphasises the importance of having started the excavation when the soil was wet and slight differences in colour showed up well: one might add that in subsoils more typical of Scotland even scantier traces have probably survived, so that few parallels may ever be found there. Scarcely less striking, though less complicated, than his similar recovery of Viking houses at Ramsey in the Isle of Man¹, this excavation has led to a report which everyone interested in prehistoric houses should read.

The house stood inside a small circular compound, and was three times rebuilt on the same general plan, all apparently in pre-Roman times. It had four elements: an

¹ *Ant. Journal*, XXIX, 1949.

inner ring of eight posts, latterly at any rate enclosing a slightly sunk area ; an intermediate ring of posts linked by a timber wall ; an outer timber wall, as much as 60 feet in diameter compared with 45 feet at Little Woodbury ; and an 'entrance hall', in the first phase 10 feet wide with double doors on the intermediate ring, leading up to the central area.

A valuable feature of the paper is the detailed consideration of how this house is to be reconstructed. Dr Bersu argues, among other things, for a floor at three successive levels, the outermost two feet above ground level—thus linking the site with his larger and later ones at Lissue, Co. Antrim and Ballacagen, Isle of Man, on which the evidence has not yet been published in full. He makes a comparative study of circular house types and, in addition, of the farming economy which may be represented at Scotstarvit.

These discussions are continued in the paper on the post-Roman Iron Age enclosure at Green Craig. Fissured rock and glacial gravel were unfavourable to excavation, but a circular house could be recognised with an outer wall of stone and timber posts, 30 feet across inside, and part of an inner ring of posts.

These reports bring out strongly the importance of *interpretation* at two separate stages in a thorough excavation, first to recognise and group the actual evidence surviving in the soil, and second to give meaning to it.

R. B. K. STEVENSON.

EL PROBLEMA DEL CHATELPERRONIENSE (AURINACIENSE INFERIOR) EN ESPAÑA. By F. JORDÁ CERDÁ. (*Cronica del VI Congreso Arqueologico del Sudeste*). Alcoy, 1950. pp. 63-7 (Pub. in *Cartagena*, 1951).

In a recent paper Francisco Jordá Cerdá has discussed the problem of the Châtel-perronian culture in Spain on the basis of his detailed studies of the collections from seven localities where the presence of this stage has been cited in the literature. Approaching the question from point of view of the concept of the survival of industrial techniques, and the blending of these with more advanced influences, he concludes that the Châtelperronian, or Lower Aurignacian of the early workers, does not exist either in the Levant or in the rest of Spain.

On the basis of Breuil's classificatory scheme for the Upper Palaeolithic¹, the following Spanish localities, sporadically distributed throughout the peninsula, have been considered as yielding evidence for a Châtelperronian occupation : the Abric Romani and Estación Agut in Catalonia, the Cueva de Benidoleig (or 'les Calaveres') in Alicante, the open-air sites of Loma del Rubio, Venta de las Navas and Llano de la Venta de las Navas in Granada, and the Caverna de Hoyo de la Mina in Malaga.

With regard to the Catalonian sites, Jordá Cerdá does not consider that a true backed blade industry is present in the upper level at the Abric Romani. Rather he feels that the assemblage from the latter horizon can best be explained as a degeneration of the Mousterian (typically represented in the lower level at this site), which has been influenced from Aurignacian (or Middle Aurignacian) sources, on the basis of the occurrence of various forms of scrapers. The nearby locality of Estación Agut, also excavated and published many years ago, has yielded a series of atypical pieces which makes its attribution to a particular stage very difficult. However, Jordá Cerdá suggests that the industry is connected with that of the Abric Romani.

The Cueva de Benidoleig, further to the South in Alicante, has yielded a clearly

¹ Abbé H. Breuil, *Les subdivisions du paléolithique supérieur et leur signification* (Congrès Internat. d'Anthr. et d'Arch. Préhist., C. R. de la XIV Session). Genève, 1912. pp. 5-78, 2ème édition, 1937.

defined Mousterian level at the base, and an upper stratum with a Solutrean industry. Although it has been claimed that the intervening horizon produced an industry of Châtelperronian type, Jordá Cerdá's impression is that here again one is dealing with an evolved Mousterian, although the quality of the material does not permit him to compare it with the corresponding horizon at the site of Còva Negra, near Játiva² in Valencia.

The fact that the three open-air localities of Granada are extremely poorly documented makes it difficult to present a critical study of the materials from them. But Jordá Cerdá remarks that there is no indication of the existence of a backed blade industry at any of them. Here again he finds that the evidence favours the view that a surviving Mousterian has been influenced by true Aurignacian, on the basis of the typology of the several series recovered.

The Caverna de Hoyo de la Mina in Malaga presents a somewhat more complicated picture. Although a few points considered by the excavators to be of pseudo-Châtelperronian type are present, Jordá Cerdá states that the industry is basically Gravettian. Also there are such typically Aurignacian forms as keeled scrapers in the same level³. But almost nothing is known concerning how long such older elements as these persisted. Furthermore, Jordá Cerdá reminds us that at Hoyo de la Mina we are dealing with an economy based on fishing and the collection of edible molluscs, rather than hunting as at most other sites of this stage, a fact of prime importance in considering the typology of the flint artifacts.

Both in the centrally located Valley of Manzanares and the relatively well investigated Cantabrian region the Châtelperronian is completely lacking. Sites in the former area show that Mousterian assemblages are overlain by Gravettian, pre-Solutrean and Solutrean levels, while in Cantabria, Aurignacian and Gravettian horizons are superimposed on the Mousterian without any intermediate level.

Jordá Cerdá does not rule out the possibility that the existence of the Châtelperronian will ultimately be demonstrated for Spain, but he emphatically states that this cultural manifestation failed to penetrate the region south of the Pyrenees on the basis of the evidence available at present. For here the chronological equivalent of the Châtelperronian of Western and Central Europe seems to be an evolved and somewhat 'degenerate' Mousterian, which persisted and ultimately became mixed with true Aurignacian elements. On this basis the Gravettian represents the earliest influence of the backed blade cultural tradition to be felt in Spain.

The importance of these facts, which indicate a more strictly limited area of distribution of the Châtelperronian, is of great significance. The problem also gives rise to interesting speculations concerning contemporary developments in North Africa, but these questions, especially the possible affinities between the developed Aterian of the Western Morocco—Tangier region on the one hand, and the Solutrean of Valencia on the other⁴, are of much too complex a nature to be discussed here.

HALLAM L. MOVIUS, JR.

² Jordá Cerdá, F., 'La Còva-Negra de Bellús (Játiva) y sus Industrias Líticas', *Arch. Prehist. Levantina*, vol. II, 1945, pp. 11-29; 'Nuevos Aspectos paleontológicos de Còva Negra (Játiva)', *Serv. Invest. Prehist.*, Serie de Trabajos, Varios, Num. 6, Valencia, 1947, pp. 19-26.

³ In this connection, the present writer has recovered several Châtelperronian points, in addition to two steep scrapers, at La Colombière (Ain) in direct association with an otherwise overwhelmingly Gravettian assemblage.

⁴ Caton-Thompson (Miss) G., 'The Aterian: its Place and Significance in the Palaeolithic World', *Royal Anth. Inst. Gt. Brit. and Ireland, Huxley Memorial Lecture for 1946*, 44 pp.

EXCAVATIONS AT NESSANA, Volume II, Literary Papyri. Edited by LIONEL CASSON and ERNEST L. HETTICH. *Princeton University Press* (London: Geoffrey Cumberlege), 1950. pp. XIV+175, with 8 plates. \$7.50 (English Price, 48s net).

This volume is the first to appear of three which will contain the report of the excavations at Nessana, the modern 'Auja-el-Hafir, in the Negeb in Palestine, conducted by Harris Dunscombe Colt, Jr. They are being published under the general direction of the Colt Archaeological Institute, of which Mr Colt is President. In a foreword he states that Volume I will be devoted to reports on the actual excavations, architecture, pottery, coins, glass and miscellaneous small objects, while Volume III will publish the non-literary papyri.

Nessana originated, it seems, as a halting place for caravans on the route from Aila, the modern 'Aqabah, on the Red Sea, to Gaza on the Mediterranean. During Roman and Byzantine times a fort here controlled the southern borders of Palestine. The place reached its greatest prosperity in the 6th and 7th centuries, despite the Arab conquest of Palestine in 634. The ruins of the old town in the plain were demolished by the Turks in World War I, but the Byzantine fortress on the hill above suffered less. The fort included a small church and from a room attached to this, used as a convenient place in which to dispose of rubbish, most of the papyri were recovered. The non-literary documents range in date from the beginning of the 6th century to the end of the 7th and the literary hands, according to the editors, fit very well into these limits. The survival of the papyri in these circumstances borders on the miraculous, but it would be foolish to expect that such a place at such a date could contribute anything important to our knowledge of Greek literature in the proper sense. Actually of the thirteen texts published no less than eight are Christian; these include portions of three codex MSS of the New Testament, viz. fragments from various parts of John, John XVI, 29-XIX, 26, and fragments from the Pauline Epistles. More exciting are fragments from a codex containing the Acts of St. George. Enough survives to show that Krumbacher's theories about this famous legend need some modification. Still more important is a sizeable fragment from the well-known correspondence between Abgar, toparch of Edessa, and Christ. This document, 'next to the *Epistula Abgari*, the fullest Greek version of the correspondence', is particularly interesting because it seems to throw light on the symbols ΧΜΓ common in Christian papyri. Three texts are concerned with pagan literature; they comprise a Latin-Greek glossary of the Aeneid, fragments of Aeneid II-VI, and a Greek glossary. The most interesting is the first. This contains a continuous text for considerable portions of Books I and II, nothing of Book III, but a selective glossary to Book IV, 248-497. It is far more extensive than any of the similar Latin-Greek glossaries of Virgil, but, like the rest, it is a rather pedestrian document, full of errors, misunderstandings, and variants, the result no doubt of many copyings by more or less illiterate scribes and of continuous use since its archetype was first composed. Nevertheless it is interesting both for comparison with the manuscript tradition of Virgil's text and also for the light which it throws on the later development of the Greek language. The editing, as of all the texts, is extremely careful and thorough and much useful knowledge may be gleaned from the commentary. Detailed criticism would be out of place. I merely observe that at 699 the supplement of the Greek for *quin* is unconvincing (perhaps *μεινυγγε*) and at 804 the scribe wrote or should have written *ἐκβρασθεντα*. The next text, fragments from Aeneid II-VI, offers nothing of particular interest nor does the fragmentary Greek glossary, a very miscellaneous and probably amateur selection from a larger lexicon. Two short legal fragments complete the list. The book is admirably produced.

E. A. BARBER.

DER RÖMISCHE SCHATZFUND VON STRAUBING. By J. KEIM and HANS KLUMBACH, (Münchner Beiträge zur Vor- und Frühgeschichte, Band 3), 4to pp. I-VIII, 1-42, pls. 1-46, figs. 1-2, 1 map; C. H. Beck'sche Verlagsbuchhandlung, Munich, 1951; Price, DM. 18.50.

The very notable hoard here described was found during building operations in October, 1950, just under two miles southwest of the Roman fort of Straubing-Sorviodurum on the Danube. It included parts of eight parade helmets, five greaves, eight chamfrons, seven bronze statuettes, four bases for statuettes, a few miscellaneous bronze objects and a very diverse mass of iron implements, including weapons, tools, hippo-sandals, fittings and nails, all found close to or covered by a bronze camp-kettle of Roman military type. The great variety of the objects indicates that they are a collection of scrap-metal, comparable in diversity with such cauldron-hoards as the Santon Downham, Carlingwark, or Blackburn Mill hoards in Britain. That some of the Straubing objects were of exceptional beauty has nothing to do with their appearance in such a hoard. But the context of the hoard is obscure. The find-spot was within 130 yards of a Roman *villa*, but in ground honeycombed with remains of more primitive settlement, and the real relationship of the objects to these adjacent structures is unrevealed.

Four of the visor-masks have something in common with the well-known British examples from Newstead, though their features, which are less stylized, with intense brows and hair blocked out in Pergamene style, owe much to Hellenistic fashion and less to Western art. Only one head-piece, from a helmet otherwise unrepresented, exhibits a coiffure readily comparable with the Newstead examples. Three further visor-masks are much more remarkable, with hair fantastically dressed in a high peaked coiffure with small tight curls and central parting, essentially Oriental in type. Their physiognomy is also Eastern. The noses display a marked hook and the mouths are small, tight and down-turned, with bronze vertical strips between parted lips, creating the illusion of wide-spaced teeth. Remarkable though the visor-masks are, the associated accoutrements are, however, no less notable and even rarer. In the Straubing hoard there are five greaves. Two are said to form a pair, and certainly bear inscriptions of *turma* or *turmae* named after the same two decurions, but the lack of symmetry in minor detail leads this reviewer at least to wonder whether they are not single pieces from two very similar sets, once allotted together to the same successive squadrons. The other three pieces are certainly single examples from lost pairs, and all were regimental equipment, issued to different squadrons or officers at different times, as is shown by the occurrence of no less than four *graffiti* on one piece. On these pieces there is full scope for a fine range of symbolic decoration. Mars, Hercules, Minerva, the Dioscuri and Ganymede, Jupiter's favourite, figure on greaves and knee-caps, all with intimate reference to the Roman army and its divine protectors. Comparable decoration appears also on the chamfrons, which comprise five and a fragment covering the full head and two which protect the eyes and forehead only. All except one of the larger chamfrons have basket-pattern eye-shields, like the actual examples from Chesters and Corbridge or the figured examples assigned to Sarmatian cataphracts on Trajan's Column. The exception has for an eye-piece a pierced Gorgon-head, and one of the smaller chamfrons has similarly a pierced head of Ganymede. The eyeshields of the smaller pieces also have a pointed terminal like those of *peltae* and one is similarly finished with an eagle's head.

The intriguing Eastern visor-masks bear no inscriptions. All the rest of the inscribed equipment, comprising one visor-mask, one head-piece, three greaves, one knee-cap, and four chamfrons, bear the names of *turmae* or their officers. The proportion is so large as to suggest that all came from a mounted regiment. Thus, while at first sight

it is tempting to connect the Oriental visor-masks with the *cohors Canathenorum milliaria sagittariorum*, the 2nd-century garrison at Straubing, there is no suggestion that this unit was *equitata* and such visor-masks seem restricted to cavalry. It is perhaps, then, possible that in the parade different teams were marked by different helmets. If this is so, however, it is odd that decisive evidence of the kind has not previously appeared, though there were, indeed, bronze faces and iron faces at Newstead.

But how came such a miscellaneous collection to form a hoard of scrap metal? The authors base upon the associated statuettes of deities, the suggestion that they had been dedicated in a shrine. But this explanation will hardly cover the miscellaneous iron objects, and it seems clear that the hoard was formed from various sources, whether by looting or by purchase as scrap-metal. If they were loot, some of the objects may indeed once have been dedicated in shrines. But if, as seems most likely, they were scrap-metal, this explanation will hardly serve, and it must be assumed that other parts of the helmets (perhaps the head-piece) had been so severely damaged as to put out of use each piece of which, significantly enough, we only have part. This observation is merely one more guess added to those already made. The outstanding value of the hoard lies in the intrinsic interest of the objects which it contains. They form an unsurpassed collection of Roman parade-armour and the authors are to be warmly congratulated upon the promptitude with which they have produced a succinct, well-illustrated and objective description of the entire collection.

I. A. RICHMOND.

THE CITY OF AKHENATEN, Part III. The Central City and the Official Quarters.

By J. D. S. PENDLEBURY. Published for the Egypt Exploration Society by Geoffrey Cumberlege, Oxford University Press, 1951. Volume I, Text, pp. xviii and 261; Volume II, plates, cxii. Price, £9 15s.

This is the third volume dealing with the Egypt Exploration Society's excavation of the great city of Amenophis IV ('Akhenaten') at El-Amarna, a site which had been explored for many years before World War I by a German Expedition, and was taken over by the Society in 1921, previous reports being published in 1923 and 1933 respectively. The present memoir is concerned with the central part of the ancient city, which contained most of the important buildings, notably the Great Temple of the Aten, the Official and Private Palaces of the King with the Royal Chapel, and the Records-Office, police and military barracks, and storehouses. Part of this area had already been uncovered by Petrie in 1891-2, and it was here that he found the famous pavement which was to many the first introduction to the characteristic art of the period. The task of the editor has been rendered exceptionally complicated by the death of Mr Pendlebury in 1941, and by the delays due to the War, so that the work of no less than seven seasons with several different field-directors has to be dealt with here; Professor Fairman and his colleagues are to be congratulated on the production of this complete and satisfactory record in very difficult circumstances.

Perhaps the most interesting features of the volume under review are the detailed plans and reconstructions of the various buildings, giving an extremely good picture of the central city as it appeared at the close of the nine years of its existence, dominated by its temples, gardens, and palaces with their gaily-decorated private apartments. Representations of such public buildings occur frequently in the tomb-chapels of the contemporary officials buried in the cliffs behind the town, which have been exhaustively copied by Mr N. de G. Davies in his six volumes of *The Rock Tombs of El-Amarna* (1903-8). It has now been found that these correspond in detail to the various parts of the Temple-complex recently discovered, of which perspective drawings are given, so that it has been

possible to identify and interpret the former in a most remarkable way. Here a word of warning may not be out of place. The visitor to El-Amarna today will be sadly disappointed if he expects to find an Egyptian Pompeii laid out for his inspection: only under the personal guidance of someone who has taken part in the excavations can he hope even to recognize the main landmarks of the city, and a far better idea of the site will be obtained by a study of the admirable air-photographs on plates XXIV and XLV.

Royal palaces in Egypt are much rarer than temples, as being built of mud-brick they have mostly disappeared completely: indeed the Great Palace at El-Amarna is the only stone edifice of the kind, and is here stated to be the largest secular building in the ancient world. By a curious coincidence a somewhat similar palace at Thebes built by Amenophis III for Queen Teye, and excavated by the New York Metropolitan Museum over thirty years ago, has just been published in instalments by Mr Hayes (see *Journal of Near Eastern Studies*, 1951), and it is instructive to compare the account of this almost contemporary building in which Akhenaten spent his youth with the constructions of his own reign at El-Amarna. At both sites an enormous number of jar-labels were discovered, many with dates and names of buildings, which are of considerable importance in supplying historical details of the period, and the main chronological results of this study will be found on pp. 152-60 of the present work; the record of the Amarna Collection at University College, London, on pp. 224-33, will also be welcomed by students to whom this material has hitherto been unavailable. The general production of these volumes is of the high standard which we expect from the Oxford University Press.

ROSALIND MOSS.

EXCAVATIONS AT OLYNTHUS. Part XIII. Vases found in 1934 and 1938.

By DAVID M. ROBINSON. (*The Johns Hopkins University Studies in Archaeology*, No. 38). pp. xix+463, with 267 plates and coloured Frontispiece. Baltimore: The Johns Hopkins Press. London: Geoffrey Cumberlege; Oxford University Press. 1950. Price, £10 net.

This volume, published partly with the aid of a grant from the University of Mississippi, is a supplement to the fifth volume of the record of the Olynthus excavations, in which the vases found in the campaigns of 1928 and 1931 were published. Those here recorded were the yield of the years 1934 and 1938. Hurriedly packed up and buried during the war, they were finally redeemed and repaired, in spite of infinite difficulties, by the labour and devotion, and mainly at the personal expense, of Professor Robinson, who may claim, like a new founder, to have restored Olynthus to modern times in these volumes, which are a veritable 'monumentum aere perennius' to himself and to the city.

More than eleven hundred vases and fragments are described and annotated and everything of significance is illustrated in the plates at the end of the book, which is produced in the sumptuous style with which we are familiar in these records. A large variety of ware is represented, both in the type of vessel and the manner of its decoration, where it has any. The ceramic finds at Olynthus extend in time from the vases and sherds of the Neolithic settlement (3000-2500 B.C.), discussed in Vol. I, to the painted pottery of 460-348 B.C., the date of the city's extinction, although there is no Minoan pottery, as there are no Bronze Age finds. Much of the pottery belongs to the period 420-348 B.C. and the fixed *terminus ante quem* has helped in dating similar material from other sites, particularly from the Athenian Agora. Vases of notable artistic merit are naturally rare, but there are a few, such as the early red-figured calyx-crater with finely drawn figures of a Nike and a warrior on one side, and a young warrior with two

spears pursuing a woman on the other. Unfortunately it is much broken, so that only portions of the figures survive. Professor Robinson claims that it is probably by the painter of the Deepdene amphora, and attributes a number of vases figured in this volume, or, more frequently, in Vol. v, to the painters 'created by Beazley'.

Professor Robinson points out that most of the Olynthian pottery is related to wares which were abundant in Rhodes and cites many parallels. Many of the vases are of local fabric and naturally the great majority of them are undecorated. A surprisingly large proportion of them, over 90 per cent, are miniatures. 'Why hundreds of miniature vases were used, I do not know', says Professor Robinson, 'unless the Olynthians, like the Japanese, served many small courses and drinks in such small vases'. This does not sound very convincing, but may possibly be the explanation of a rather puzzling fact.

The plain ware by its abundance and variety affords ample material for the study of the development of the shapes and forms of vases in the late 5th and 4th century up to 348 B.C. and Professor Robinson discusses many types: oenochoae, canthari, scyphi, pyxides, lecythi, asci and others. It would be superfluous to praise either the skill and learning of the author or the magnificence of the production. G.F.F.

EXCAVATIONS AT GÖZLÜ KULE, TARSUS; the Hellenistic and Roman Periods. *Vol. I, Text, pp. 420; Vol. I, Plates, 276 Pl. and 6 Plans. Edited by HETTY GOLDMAN. Princeton University Press (London, Geoffrey Cumberlege), 1950. Price, £11 15s. od.*

In these two sumptuous volumes we have the final official report of the American excavations at Tarsus in so far as the Hellenistic and Roman periods are concerned. The real purpose of the expedition had been to trace the material cultures of the Cilician plain from the Neolithic down to the end of the Hittite age, for which purpose the site chosen was the great prehistoric mound that rises on the outskirts of Tarsus; that mound formed no part of the famous classical city and was occupied, when at all, only by workshops and humble dwellings, so that no treasures of Hellenistic or of Roman art could be forthcoming; in such circumstances Miss Goldman has all the more reason for feeling pride that her expedition, seeking primarily prehistoric material, should have made its contribution to the classical archaeology of the Near East.

The buildings, originally poor, and terribly cut about first by Islamic builders and, in recent times, by the defence-works of the French troops, were of interest only as yielding a certain amount of dating evidence in a site where stratification was always difficult and often altogether lacking; Miss Goldman's extremely careful methods of field work have wrested from what might have seemed hopeless confusion an ordered chronology which, where it applies at all (and she is properly cautious in its application) cannot be gainsaid; and it is of course this that gives value to her finds in general. The fixed dating of her 'units' is, naturally, based on the coins; of these we have a very full discussion by Miss D. H. Cox which brings out some valuable information regarding the local mint. Different sections of the book deal with the lamps, the stamped amphora handles, the pottery, the figurines and the inscriptions—the last being, as one would expect, few in number and with one exception of little importance. The terra-cotta figurines form an interesting series, the more so because they seem to be nearly always of local make, so that they illustrate the output of one of the fairly numerous provincial factories engaged in this popular trade; on the whole, as Miss Goldman points out, the Tarsus koroplasts were more closely allied in taste and technique with those of Alexandria than with the more sophisticated craftsmen of Myrina and Tanagra. Of the

pottery too a great deal is of local manufacture, sometimes imitating foreign imports, Attic or other, but sometimes carrying on a local tradition.

Miss Goldman, speaking of the plain lamps, remarks that the trend towards subtle type analysis is assuming some alarming aspects, a remark with which the reviewer heartily agrees; but the elaborate detail of her own publication is almost equally alarming. The catalogue of pottery (mostly fragments) takes 86 pages—'all pieces which were given inventory numbers at the excavation are included, even though subsequent finds have eclipsed their original importance'—and they are illustrated by 90 plates of which 50 are photographic. The very full general account illustrated by a selection of typical examples and supplemented by a stratification analysis showing types and numbers instead of catalogue references would have done full justice to what is after all second-class material. The lamps and the terra-cottas are of course more individual but not all the fragments are important nor are slight variations always informative; 20 plates with 50 pages of text for the former and 45 plates with 86 pages for the latter are far too generous an allowance; incidentally, the reader would be helped if scales were given on the plates instead of measurements in the catalogue. The serious objection to this luxury of detail is not merely that the student is suffocated by information, a great deal of which has literally no value; it is that it puts the book out of the ordinary student's reach. There is here a contribution to classical knowledge, but it is not one for which the scholar should be asked to pay £11 15s 0d; the object of us all is the diffusion of knowledge, but these extravagant prices, out of all relation to real values, defeat that object most grievously.

LEONARD WOOLLEY.

THE VEDIC AGE. (Vol. I of History and Culture of the Indian People. Bhāratiya Itihāsa Samiti). General Editor: R. C. MAJUMDAR. London. George Allen and Unwin Ltd. 1951. pp. 1-565. Pls. I-VIII, 2 folders, 2 maps in text. Price, 35s.

This volume of some five hundred pages of text is written exclusively by eleven Indian scholars, and it is of great interest on this account no less than for its subject matter. The volume is divided into seven Books dealing with I, Introductory; II, The Prehistoric Age; III, The Aryans in India; IV, Historical Traditions; V, The Age of the Rik-Samhita; VI, The Age of the Later Samhitas; VII, The Age of the Upanishads and Sūtras. Each Book contains from two to six chapters. There are copious notes and bibliographies.

In the Foreword, the intention and general plan of this and the subsequent volumes is discussed by K. M. Munshi, and in the Preface, by the general editor, R. C. Majumdar, some useful points are made about the nature of Indian History and the misleading practice of dividing it into Hindu, Muslim, and British periods. He proposes the substitution of the terms Ancient, Medieval, and Modern, and suggests that the boundary between the first two should be approximately A.D. 1000, a time of internal decadence and the intrusion of foreigners. The same writer deals with other preliminary matters in the first two chapters. Chapter III consists of an excellent historical sketch of archaeological work in India up to 1944. It was written by the late Rao Bahadur K.N. Dikshit, sometime Director-General of the Archaeological Survey of India, and in describing Prinsep's decipherment of the Asokan inscriptions in 1837, he rightly says it was one of the great epigraphical achievements comparable to the elucidation of hieroglyphic or cuneiform. The next three chapters provide welcome summaries of the geology, geography, flora and fauna of India as background material for the human scene.

It is in Book II, The Prehistoric Age, that a most unfortunate but doubtless unavoidable matter comes to light. The archaeology is out of date in regard to information and

largely in treatment as well. The outstanding achievements obtained during Dr Mortimer Wheeler's tenure of office as Director-General of the Archaeological Survey of India were evidently published after the main writing of the text, although an appendix to the chapter on the Indus valley civilisation quotes references in *Ancient India* No. 3 (1947). It is, however, of value to read the portions on the correspondences between Harappā civilisation and cult and early Hinduism. Dr Sankalia's account of the Indian palaeolithic is more satisfactory than the later part of his chapter which comprises sections on 'neolithic' and 'copper' ages and, in general, readers will have to turn to Professor Piggott's book (reviewed above) for all archaeological matters. The chapter by S. K. Chatterji on Race Movements and Prehistoric Cultures is one, however, of considerable interest and is clearly set forth. It includes a summary and discussion of Indian linguistic history, racial types, and the origin of the Hindu pantheon. Prof. Chatterji makes a good case for the Harappā civilisation having been Dravidian, and he stresses the basic nature of the Austro-Dravidian element in all subsequent Indian civilisation. One matter of special interest is the possibility of racial and linguistic connections with ancient Mesopotamia. The extraordinarily Indian physical appearance of, for instance, the well-known stone figure in the British Museum of an un-named governor of Lagash, has for long struck the present writer, and in philology some further exploration into the possible connections or borrowings of Dravidian seem desirable. Prof. Chatterji quotes the word *ur*, 'towns', from a list of early Dravidian words.

Book III is devoted to the general consideration of the Aryans in India, and three of its four chapters are written by B. K. Ghosh of Calcutta, one of the most lucid and informative contributors to the volume. In Chapter x he gives a very full historical sketch of the growth of Indo-European comparative philological studies, and the conflicting views that have held the field from time to time. It is interesting to be reminded that it was a 16th century Florentine merchant at Goa who first claimed that a relationship existed between Sanskrit and certain European languages. Ghosh considers only the philological links in the Indo-European problem, and does not appear to know of the work of Vendryes on parallels in religious terminology, and the sociological and mythological correspondences that have been studied by Dumézil and Dillon, amongst others. He seems to think that it is generally accepted that the Mycenaean did not speak Greek, but the opposite view is in fact held by the best authorities.

Chapter xi is a short but concentrated discussion of the relationship between the Iranians and Vedic Aryans, and many stimulating, if not wholly conclusive, views are offered. The twelfth chapter, also by Ghosh, is an admirable summary of the nature, arrangement, and relative chronology of the Vedic literature. It would have been of great additional interest if some account had been given of the way in which oral tradition was handed down, and what use, if any, was made of written texts until recent times.

A. D. Pusalker contributes Chapter xiii and reviews the geographical and tribal names in the Rigveda and possible modern identifications. The same writer is responsible for the two chapters that comprise Book iv. The Historical Traditions are mainly derived from a body of lore known as the Purānas which are admitted to be, in their present form, some two thousand years later than the earliest events they purport to record. The texts of the Purānas are very corrupt, but are claimed to be not wholly unreliable, and many pages are devoted to setting out a traditional scheme of dynasties and their alleged chronology. The earliest dates are placed in the late fourth millennium B.C., and continue down to include the Bharata War at c. 1400 B.C. The whole subject is set forth as if the chronology was meant to be taken seriously, but it is not clear if Dr Pusalker was merely at pains to elaborate a curiosity, and surely, if the Aryan or

non-Aryan dynastic histories meant anything for the third and second millennia B.C., some correlation with the Harappā civilisation would emerge. However, the subject is presented as in a water-tight compartment and in its present form invites no confidence.

The last three Books in the volume deal in some detail with the language and form of the various components of Vedic literature, and there are chapters on political and legal institutions, religion, and social conditions. The contributors are B. K. Ghosh, V. M. Apte, and M. A. Mehendale. Ghosh considers, as others do, that inhumation burial was practised as well as cremation in Rigvedic times, but the passage he quotes about the removal of the bow from the dead man's hand would suggest that this was because he had in fact to be burnt, otherwise he would surely have brought it with him into the grave. The chapters by Apte on social and economic conditions are all too brief for their extraordinary interest and value to archaeological studies far beyond India, no less than within it. One would like to know more about the 'kettles' used for cooking. These presumably are bronze cauldrons, and are an important indication of the techno-cultural horizon of the Aryans. Other matters of special interest are the strongholds (*pur*), and the carts, no less than the chariots. The special characteristics of the *Sūta*, 'charioteer', who drove the vehicle for the warlord finds parallels in Greece and Ireland.

The Vedic Age will undoubtedly do much to increase interest in early Indian studies; it demonstrates, too, the enormous opportunities for further research and competent exposition by Indian scholars, to whose increasing activities students everywhere will wish well.

T. G. E. POWELL.

THE ROMAN STAGE. A Short history of Latin drama in the time of the Republic.

By W. BEARE, M.A., Professor of Latin in the University of Bristol. pp. xi, 292, with 8 halftone plates and 8 text illustrations. London. Methuen and Co. 1950. Price, 25s net.

Professor Beare, whose name has long been associated with Plautine studies, presents in this volume a study of the development of all types of Latin drama in Republican times from tragedy to popular farce, considered in relation to the theatre for which it was intended. The difficulty in writing on this theme is that, apart from the plays of Plautus and Terence, the material that remains for us is meagre in the extreme. For example the known writers of *fabula togata*, 'comedy in native dress', are Titinius, Afranius and Atta. Of Titinius there survive 15 titles and 180 lines, of Afranius there are 44 titles and some 400 lines, of Atta 11 titles and 20 lines.

Professor Beare does his best with these slender resources and conveys a faint flavour of past drolleries in the renderings of some of the titles: *Lucubratio*, 'Up by lamplight', *Tiro Proficiscens*, 'Off for his first campaign', *Matertera*, 'Auntie', *Icta*, 'She who gets slapped', but has to admit that 'our main evidence, that of the titles and fragments, does not enable us to distinguish clearly between the work of these different writers of *togatae*, or to reconstruct with probability as much as a single plot'.

With tragedy we are in even worse case, for in this field there is nothing to correspond to the works of Plautus and Terence. The plays of Seneca are of doubtful relevance, for they are not only of a later age but are clearly intended for reading and declamation, rather than for stage production. From the earlier period, when tragedy actually occupied the stage and was a live force, we have nothing but titles and fragments. Of Ennius there are 20 titles and 400 lines, of Pacuvius about the same number of lines with twelve titles, and Professor Beare has to make the sad admission that 'in the whole range of Latin drama we have no knowledge of the course of as much as one plot invented by a Latin writer'. Yet in spite of these limitations the study of Latin drama retains a lasting

interest, and the plays of Plautus and Terence gain an enhanced importance as the only complete representatives of the New Comedy that flourished in the Greek world of the 3rd century B.C., and the main links in the literary tradition of dramatic entertainment. The discovery in modern times of considerable fragments of the plays of Menander, although it has given us a firmer basis for judgment, does not affect the position of the Latin writers of Comedy as the main vehicle for this literary form for later ages. Supposed rules of composition, drawn up by late Latin grammarians like Donatus, had immense influence on European drama, but Professor Beare shows, from the evidence of the extant plays and from what we can glean of stage conditions, upon what a slender basis they often rest. In an interesting chapter he discusses the law of five acts, which can claim the authority of the *Ars Poetica*, and concludes that 'the five act theory seems to be nothing but the product of Roman pedantry, based ultimately on Horace's practical hint—possibly derived from some remark of Varro'.

The book is packed with information, but very readable, and is an admirable compendium of all matters connected with the Roman stage—the dramatists and the extant remains of their work, the theatre, stage buildings and conditions of production. Illustrations of vases and architectural remains are used in evidence and the whole treatment is critical and scholarly. In many of these matters the absence of exact knowledge has given rise to a large literature on controversial questions which Professor Beare handles with a skill that avoids dullness. Some of the more technical subjects, such as the change of scene and scenery, the 'angiportum', the doors on the stage and the curtain, are treated in appendices, consisting of papers by the author reprinted from Classical periodicals. The book can be strongly recommended to all students of Roman life and letters.

G.F.F.

EXCAVATIONS AT ISLAND MACHUGH. By O. DAVIES. Supplement to *Proceedings of the Belfast Natural History and Philosophical Society*, 1950, pp. 124, figs. 47, pl. 8. Price, 15s. *Northern Whig*, Belfast.

What Dr Davies describes as the last major report on his archaeological work in Ireland, deals with the important sequence of stratified deposits uncovered by careful excavation on an island about 180 by 120 feet across, situated just west of the centre of Ulster.

A specialised and probably late form of Neolithic B pottery was freely used by the first inhabitants of the natural tree-covered island. Long cord impressions are a feature of the decoration. The commonest flint tool was the hollow scraper. Axes were scarce, but one wonders what evidence there is for the suggestion that most stone axes in the British Isles were hoes. The small diameter of the numerous remains of posts, associated with cobbling, may indicate flimsy structures. No plan, however, was recognisable. Dr Mitchell's pollen analysis places this occupation at the end of the Sub-boreal climatic phase (Sub-zone viib), indicating that it is no earlier than some finds of Early and Middle Bronze Age date from the south of Ireland.

After an interval when trees grew undisturbed, users of Late Bronze Age pottery, including a cordoned urn, constructed a palisade wall round the island, and laid sand and cobbles on a foundation of brushwood over most of the interior with planking and brushwood alone at the edges. Scattered over the area were found stumps of posts, and near the centre a large hearth. But no picture emerges of the roofed structure(s) that may be presumed, particularly from two squared beams continuously slotted for upright boards, and others with mortise holes. The excavator suggests that the greater part of the area was roofed except where a platform had been formed by tumbling logs

into the water at one corner of the island. A bronze socketed axe and socketed sickle should belong to this phase.

Palisading, posts, plank and brushwood floor, were repeated at some later date, to be estimated from the conflicting finds : parts of two ' souterrain ' pots dated elsewhere to A.D. 500-1000, together with an equally late type of wooden whorl and iron nails, have to be balanced against fragments of a late ' cinerary urn ', with typical cord decoration but no cordons, and other sherds of bronze age fabric. Dr Davies estimates a date contemporary with the middle or later Roman Empire, partly on the strength of a distant analogy with pottery of bronze age tradition at Traprain Law. All this is considered under the heading ' Bronze Age Finds ', leaving Early Christian for the next stratum, ill-defined in forest peat. For the outsider it is always hard to believe that Irish culture and chronology followed rules of their own, though the view is widely held in Ireland.

After a further interval hearths, iron slag, small moulds and crucibles indicate medieval metal workers who used quantities of shouldered jars with decoration usually confined to the flattened rim. This pottery is widely distributed in Ireland and known as ' crannog ware '. But at Island MacHugh its stratification has been observed for the first time. Not only did it underlie the rubble and mortar tower known to have been built early in the 15th century, but large amounts were found in the upper levels latterly associated with glazed sherds. Thus it seems to have continued for centuries with little recognisable change, at least down to the end of the 16th century. In 1603 the castle on Island MacHugh was ' raised to the ground '—in fact, as often, only slighted—and traces of 17th century occupation were less frequent than of squatters of about 1750-1820.

In an interesting discussion on the suspension of prehistoric round-bottomed pots, Dr Davies considers that they were too fragile to place within a foot of burning wood and contrasts the rims and shoulders of Neolithic A pottery, designed, he thinks, to hold a strap, with the necks of Neolithic B, grooved for a thong or cord.

Such discussions are few, however, and the reader must be prepared for a recital of bare finds and facts whose interpretation is hardly indicated, as in the case of the structural remains or two pages of data on phosphate content. This is no doubt partly due to the pioneer nature of the work, and even more to the unfortunate circumstance, stated in the preface, that the report had to be written abroad without access to the finds, and from field notes and drawings hurriedly executed. Yet the many drawings of pottery could have been greatly improved, if made simply as diagrams without shading or indications of texture, and might have been grouped in the order of the text. It is more regrettable still that the plans are so printed as to make many of the distinctions of period indecipherable. Most purchasers would surely have readily paid a higher price to ensure adequate paper and reproduction, and to be spared having to have the volume sewn before use.

R. B. K. STEVENSON.

EXPLORATIONS IN EASTERN PALESTINE, IV : part 1, text ; part 2, pottery, notes and plates. By NELSON GLUECK. Published by the *American Schools of Oriental Research, Annual*, vols. XXV-XXVIII (for 1945-9). Publisher's address—Drawer 93a, Yale Station, New Haven, Conn. Price 12 dollars (2 vols, postage 28 cents).

This last instalment of Nelson Glueck's *Explorations in Eastern Palestine* gives the results of his inspection of the area north of the River Zerga and in the Jordan Valley.

The book is primarily an inventory of sites. But it is interspersed with the author's reflections on many questions of history, topography and archaeological method. It can thus be described, aptly enough, as a mine of factual material and of earnestly pondered conclusions, both, as is the way with mines, rather troublesome to extract.

Dr Glueck's main theme concerns the fluctuations of urban settlement, and their dependence on human rather than climatic or geographical factors, which he believes to have been constant. Given conditions of peace, intensive settlement based on cultivation has always been possible, both in the highlands and in the Jordan Valley, and has been achieved in certain 'peak' periods of which surface remains are an index.

Arising out of this general theme, space permits only a few points to be noted.

The dolmens, so abundant in northern Gilead, denote a population of 'skilled artisans' which 'spilled over into marginal areas' and developed 'an advanced agricultural civilization' in the 6th millennium 'at the latest'. It would have been interesting to know Dr Glueck's own reasons for now dating the dolmens so early, especially in view of the coincidence noted on p. 75 between their distribution and that of the Chalcolithic settlements. To say that the discovery of Early Bronze Age pottery in some of them 'has no bearing whatsoever upon the dating of the dolmens' (p. 387) is hardly correct; at least it gives a *terminus ante quem*, disposing, for example, of the author's own earlier dating about 2000 B.C.

Of the 'amazing Nabataeans' Dr Glueck concludes that the northern extension of their kingdom was mainly commercial and that while in the south they irrigated lands never cultivated before or since, in the north 'Hauran and Jebel Druze could not attract land-hungry Nabataean farmers in the same manner'.

On historical or geographical probabilities this may be true, but it does not follow, as Glueck suggests, from the mere absence of that most specialized and least transportable of pottery types, the painted ware so common on Nabataean sites in southern Transjordan.

There is no question that Dr Glueck's survey is an indispensable source of material for the earliest history of Transjordan (no pottery is illustrated later than the Iron Age, and very little of that). Any future synthesis will certainly incorporate many of his conclusions. The identifications of biblical sites in the Jordan valley are, also, convincingly demonstrated. It is a pity that reading the book is so like exploring the author's field notes, even down to the abbreviations. (n for 'north', HM for 'hand-made', and so on).

Discussions of general conclusions, valuable in themselves, are scattered through the book as in a bran-pie, the same points recurring in almost identical phrases in widely dispersed contexts. (cf. p. 326, n. 886 with p. 396, n. 1125; or pp. 191-2 with p. 387). The index is little help, for besides lacking all sub-headings it is far from complete. Thus under 'Chalcolithic' the important pages 74-5, and many other pertinent passages, are omitted; and under 'Hellenistic' the paragraph on p. 353.

The author's own summary, a single short page with no references to the foregoing chapters, is inadequate; by absorbing some of the preceding discussions it could usefully have been expanded into a full chapter.

Facts are no easier to get at. The text, with rare exceptions, gives no references to the plates, and the plates none to the text. Between them, the only link, yet a formidable barrier, stands an array of indices, guarded by an impenetrable alphabetical order. This attempts, in defiance of all experience in listing Arabic place names, to give due honour not only to such optional prefixes as 'tell' or 'khirbet' (accounting for four-fifths of the names), but also to the definite article with all its assimilations. Consequently a site like Khirbet (or Tell) Sheikh Muhammad can be listed under the K's on pp. 687 and 702, but under the T's on p. 498. All of which makes the book so much harder to use.

The first thing one looks for in a work of this kind is a good map. It is disappointing to find that the pocket at the end of Part II contains only a blank sheet sprinkled with

numbers and three place-names. What a pity that room could not have been found (perhaps by scrapping half the photographs of potsherds, made redundant by the excellent and far more informative drawings) for a fully detailed map, in four sheets if necessary, covering the whole area surveyed (which this does not) on a scale large enough for names and contours to be shown, not to speak of rivers, roads and boundaries. Without such a map, for which the air photographs only occasionally compensate, most of the topographical discussion, which forms an essential part of the book, is impossible to follow.

Criticism of the manner of presenting some of the materials here assembled cannot detract from the solid merit of a survey so perseveringly carried out in difficult country. All archaeologists will recognise this book, and the years of pioneer work in the field which it represents, as an outstanding personal achievement.

R. W. HAMILTON.

L'ÉVOLUTION DE LA CARTOGRAPHIE DE LA RÉGION DU MONT-CENIS ET DE SES ABORDS AUX XVe ET XVIe SIÈCLES. Étude critique des méthodes de travail des grands cartographes du xci^e siècle. By M.-A. DE LAVIS-TRAFFORD, pp. 125; 12 plates. Chambéry: Librairie Dardel (London: Sifton Praed), 1949.

The watershed of the Graian and Cottian Alps, forming the frontier between Dauphiné, Savoy and Piedmont, is a region of violent relief in which the pattern of human settlement and movement is controlled by the upper courses of the rivers—the Isère, the Arc, the Dora Riparia—and by the passages across the mountain barrier. In tracing the ancient routes between 'the two Gauls' which traversed the watershed Dr de Lavis-Trafford has had occasion to search for material in early maps as well as literary and archaeological evidence. This monograph is a by-product of his wider study of the old mountain roads between the Haute Maurienne and the Susa Valley.

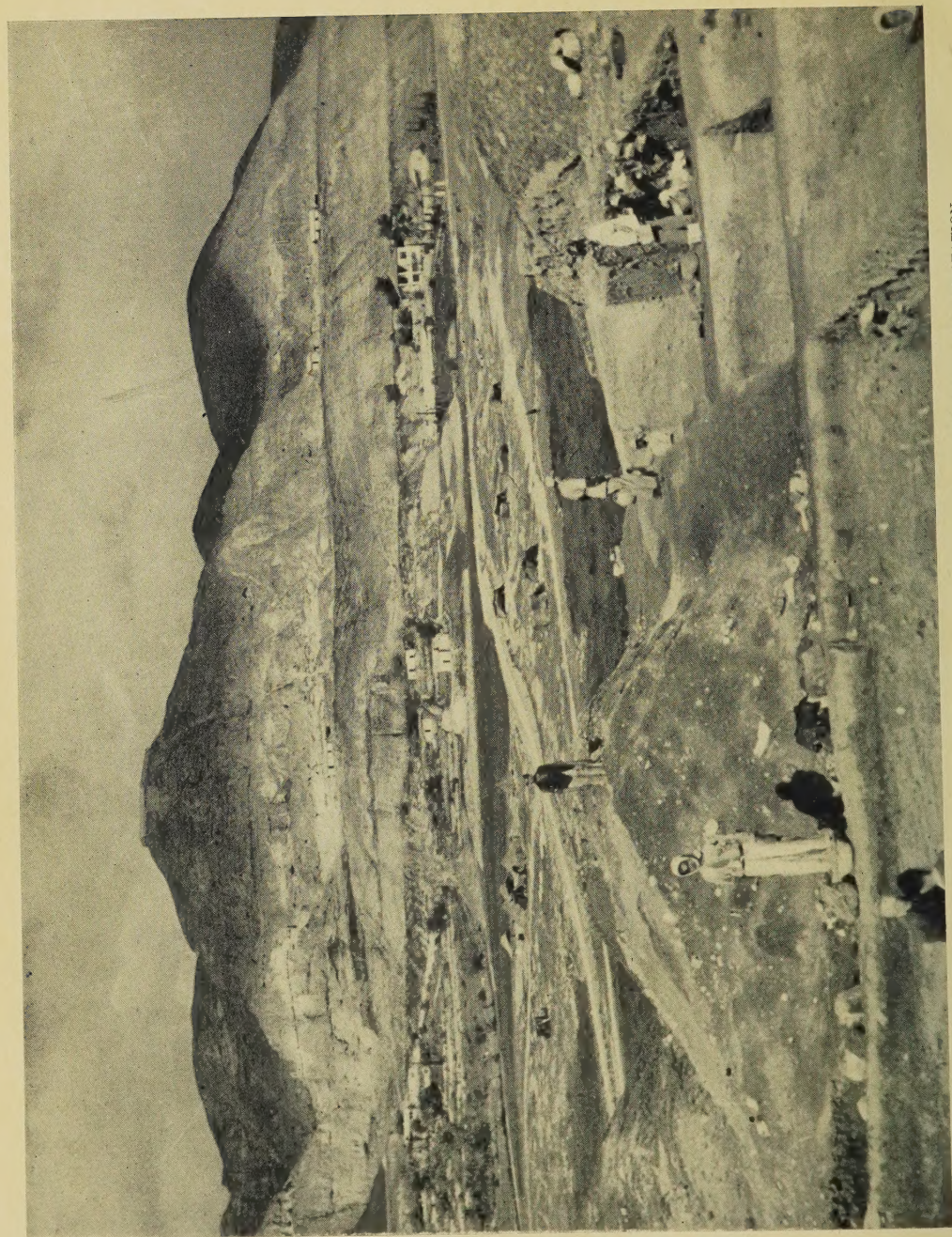
To his scrutiny of the representation of the Mont Cenis region in maps of the 15th and 16th centuries Dr de Lavis-Trafford brings an intimate knowledge both of the country and of its archives and literature, manuscript and printed. He rightly supposes that a comparative study of contemporary topographical texts can throw light on the methods of compilation employed by early cartographers. All the maps which he examines, from the printed Ptolemies of the 15th century to Magini, were made by 'géographes de cabinet', or armchair geographers; none is from survey. They are therefore a proper subject for criticism of this kind. Confronted with the topographical knowledge available in their time, the cartographers cut a poor figure. They are convicted, in general, of a superstitious faith in ancient authorities and a frivolous neglect of contemporary sources of information; of an altogether uncritical attitude in weighing and assimilating evidence; and of arbitrary deformation of the hydrography and relief of the region. In the work of individual cartographers, Dr de Lavis-Trafford detects discrepancies between a map and a text by the same author (Signot, 1515) and between maps from the same hand (Gastaldi, 1555 and 1559); inadequate supervision of the engraver (Ortelius); and the exercise of fantasy in filling lacunae due to ignorance of readily accessible information (Mercator). In the period reviewed, Gastaldi's map of Piedmont (1555) is shown to give the most satisfactory version of the Haute Maurienne and of the Mont Cenis plateau: the source of the Arc is correctly identified, and two Monts Cenis are distinguished although misplaced. Gastaldi unexpectedly fails in his delineation of the Italian side of the watershed (the upper valley of the Dora Riparia), for which Orance Finé's map of France (1525) remained the most accurate until Magini's 'Piemonte et Monferrato' of 1608. That the region under examination lies at the extreme limit of political territories (France-Italy; Savoy-Piedmont) partly explains

weaknesses in its representation, for an early cartographer's work is often most careless or perfunctory at the edges of his map.

Dr de Lavis-Trafford's detailed analysis of the maps is admirable, and his readers should be grateful for his precise indications of size, method of engraving, present location, and bibliography. His neglect of scale is curious, for this factor governs the selection and delineation of detail in small-scale maps, especially those engraved on wood with their necessarily coarser line and lettering. The scale of no map is given, and the word 'échelle' occurs only twice in his text.

In contrasting the reputations of Gastaldi, Mercator, Ortelius with their patent failures in his field, the author assumes the accents of the little boy who remarked that the Emperor had no clothes on. His argument from the particular to the general, however, rests on too narrow a foundation to command unqualified acceptance. We do not believe that there is a conspiracy to exaggerate the critical sense or scientific discipline of these cartographers. A fair assessment must take account of the familiar fact that the maximum of critical effort is commonly applied at the periphery of knowledge, at the frontier between the known and the unknown. We must mitigate Dr de Lavis-Trafford's uncompromising judgment if we recall (for instance) the care with which the crucial northern passages were drawn and revised on maps, from the most recent reports of explorers.

Nevertheless the author has done a useful service in reasserting certain first principles neglected at their peril by historical geographers and others who use maps as evidence. Maps and documents are complementary, and maps alone are no true index of the state of topographical knowledge at their date. A chronological series of early maps will not necessarily show continuous progress in representation; its character may be static or even retrogressive. Maps are a conservative, indeed a reactionary, element in the development of geographical ideas: Richard Gough in 1780 estimated that 'the several sorts of . . . [maps] are supposed to amount at least to 16,000; but of these not above 1,700 are originals'. Dr de Lavis-Trafford notes the absolute character of statements made by a map in comparison with those of the written word which can more easily be qualified. In ascribing reasons for the survival of errors in maps, he rightly points to the authority lent by the name of a Gastaldi or a Mercator even to their inferior work, but he does not sufficiently emphasise the force of two other factors, the one physical the other economic. These factors are the immensely long life of copper plates and the consequent temptation to pull and sell impressions long after their geographical content had fallen behind contemporary knowledge. A comprehensive study of commercial organisation and relations in the map-publishing industry would throw much light on this and similar problems in the history of cartography. R. A. SKELTON.



JERICHO: LOOKING WEST FROM THE SUMMIT OF THE TELL TOWARDS THE MOUNT OF TEMPTATION